AMERICAN JOURNAL *of* PHYSICS

A Journal Devoted to the Instructional and Cultural Aspects of Physical Science

VOLUME 18, NUMBER 9 DECEMBER, 1950

Studies of Nuclear Energy Levels with Fast Neutrons*

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OUR present understanding of the structure of atoms, in particular of the mechanics of the motion of the atomic electrons, was derived primarily from investigations of energy levels of the electrons as obtained from studies of atomic spectra. The success of this approach in the atomic case suggests the possibility of gaining information about the structure of the nucleus from a study of nuclear energy levels.

How energy levels may be observed in nuclear reactions may be seen with the aid of Fig. 1. When a stable nucleus is bombarded by a particle, this particle may amalgamate with the nucleons of the bombarded nucleus. The unstable nucleus so formed is referred to as the compound nucleus. This compound nucleus may go over into a stable configuration either by re-emitting the same kind of particle by which it was formed (scattering) or by emitting a different type of radiation. The presence of an energy level in the compound nucleus usually manifests itself by an increased probability of the occurrence of the reaction when the energy of the bombarding particle has the right value to form the compound nucleus in one of its excited states. In the center portion of Fig. 1, the variation with energy of the probability or cross section for the reaction resulting from this effect is indicated schematically.

Energy levels in the final nucleus may be studied by observing the energy distribution of the outgoing radiation. This radiation may be composed of groups of discrete energies, even when the bombarding particles are monoenergetic. Such groups are indicated in the right-hand

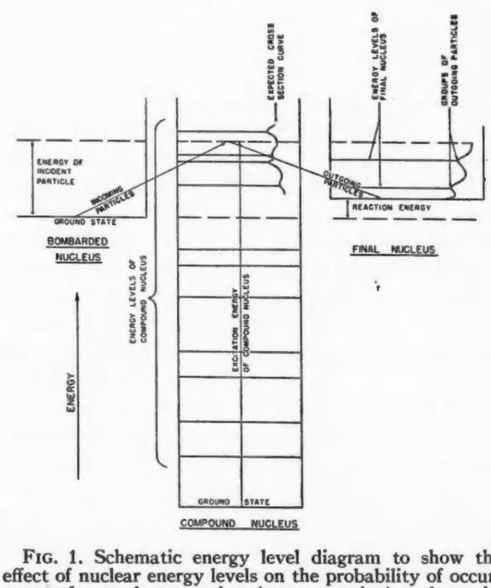

FIG. 1. Schematic energy level diagram to show the effect of nuclear energy levels on the probability of occurrence of a nuclear reaction (center portion) and on the energy distribution of the reaction products (right-hand side).

* Based on a paper which was given at the Chicago meeting of the American Physical Society on November 25, 1949.

† The work described in this paper was supported partly by the AEC and partly by the Wisconsin Alumni Research Foundation.

portion of Fig. 1. Since the binding energy of a nucleon is usually of the order of 8 Mev, observations on the compound nucleus will reveal only energy levels at a high excitation energy, while information about lower lying levels may be obtained from measurements on the final nucleus. A disadvantage of observations on the final nucleus is, however, that selection rules will in some cases prevent the appearance of all the energy levels, whereas all the levels of the compound nucleus (which can be formed by bombarding particles of sufficiently low angular momentum) should be observable. The experiments to be described in this paper are concerned only with observations on compound nuclei.

The experimentally simplest procedure for observing nuclear energy levels is to bombard a stable nucleus with charged particles, such as protons, and study the resulting interactions. Especially in the heavier nuclei, however, the predominant interaction will be the electrostatic repulsion which may mask the specific nuclear interaction which is of primary interest to the nuclear physicist. As a consequence, the use of uncharged particles, i.e., neutrons, has yielded valuable results in spite of the fact that neutrons cannot be accelerated directly, cannot be collimated easily, and are more difficult to detect. In fact, most of the available information about level densities in intermediate and heavy nuclei was obtained from neutron experiments, in particular experiments from using slow neutrons.

Excellent techniques have been developed for studies of nuclear energy levels by means of slow neutrons up to energies of about 5 kev, but the limitation of these techniques to an energy interval of only 5 kev at an excitation energy of the compound nucleus of usually about 8 Mev is a severe disadvantage. The experiments to be described in this paper are essentially an extension of the cross-section measurements which had previously been performed with slow neutrons to higher energies. The results, which will be used as illustrations, were obtained at the University of Wisconsin during the period from 1947 to 1950. Very similar measurements were carried out earlier by MacPhail[1] at Princeton, by Aoki[2] in Japan, and particularly by Williams' group[3] at Minnesota. No attempt will be made in this paper to give a complete review of the field;[3a] the emphasis on data obtained at this laboratory is not intended to imply that results from other laboratories are less interesting or less reliable. Only the Minnesota group has carried out measurements in the same energy range and in some cases for the same elements as those investigated at Wisconsin. Wherever a comparison of the results is possible, the agreement between the data obtained at the two laboratories is excellent.

[1] M. R. MacPhail, *Physical Rev.* **57**, 669 (1940).
[2] H. Aoki, *Proc. Phys. Math. Soc. Jap.* **21**, 232 (1939).

Theory of Nuclear Resonances

When the incident particle has an energy which will lead to the formation of a compound nucleus at an energy close to one of its energy levels, the resulting interaction is usually referred to as a resonance phenomenon. The theory of nuclear resonance was developed by Breit and Wigner[4] in 1936, and has since been further developed by these authors and others.[5] For the purpose of the present discussion it will be most convenient to present the Breit-Wigner theory in the notation of Feshbach, Peaslee, and Weisskopf.[6] If a neutron of energy E and wave number $k(=2\pi/\lambda$, where λ is the wavelength) is incident upon a target nucleus of spin I, the cross section for absorption of the neutron for energies in the neighborhood of an isolated resonance which occurs at a neutron energy E_r is given by

$$\sigma_a=\pi k^{-2}\frac{2j+1}{2(2I+1)}\frac{\Gamma_n\Gamma_a}{(E-E_r)^2+(\Gamma_n+\Gamma_a)^2/4}, \quad (1)$$

where j is the spin of the compound nucleus, Γ_n the width for re-emission of the neutron, and Γ_a the width for absorption. (The width for a process is a measure of the probability of the occurrence of the process, or, more precisely, is equal to $h/2\pi\tau$, where h is Planck's constant and τ is the mean life of the state for decay by the process considered.)

[3] Bailey, Bennett, Bergstralh, Nuckolls, Richards, and Williams, *Physical Rev.* **70**, 583 (1946); Nuckolls, Bailey, Bennett, Bergstralh, Richards, and Williams, *Physical Rev.* **70**, 805 (1946).
[3a] For a summary of neutron cross section measurements see R. K. Adair, *Rev. Mod. Physics* **22**, 249 (1950).
[4] G. Breit and E. Wigner, *Physical Rev.* **49**, 519 (1936).
[5] E. P. Wigner, *Am. J. Physics* **17**, 99 (1949).
[6] Feshbach, Peaslee, and Weisskopf, *Physical Rev.* **71**, 145 (1947).

Besides being absorbed, it is possible that the neutron will be scattered by a nucleus elastically, i.e., without loss of energy. Elastic scattering may be described as consisting of two parts, nonresonant or potential scattering and resonance scattering. The potential scattering can be interpreted most easily by analogy to the scattering of light as the interaction of the neutron wave with a rigid sphere, and the probability of the interaction as a function of wavelength and scattering angle may be calculated exactly as in the optical case. It is most convenient to consider separately incident neutrons of different orbital angular momenta. The cross section of a nucleus for potential scattering of neutrons of l units of angular momentum is given by

$$\sigma^l{}_{\mathrm{Pot}} = 4\pi k^{-2}(2l+1)\sin^2\delta_l, \tag{2}$$

where the phase shifts δ_l are given in terms of the nuclear radius R by

$$\begin{aligned} \delta_0 &= kR \\ \delta_1 &= kR - \pi/2 + \cot^{-1}kR \\ \delta_2 &= kR - \pi + \cot^{-1}(k^2R^2-3)/3kR. \\ &\cdots \end{aligned} \tag{3}$$

According to Eq. (3) for low neutron energies or $k \ll R$ the potential scattering is almost entirely due to neutrons of zero orbital angular momentum (s-neutrons), while at higher neutron energies higher angular momenta become important. The cross section for potential scattering for neutrons of all angular momenta is $\sigma_{\mathrm{Pot}} = \sum_l \sigma^l{}_{\mathrm{Pot}}$. It should be pointed out that Eqs. (3) are derived on the basis of the rigid sphere model of the nucleus. Since this model is certainly only an approximation, these equations are probably not strictly valid.

Superimposed upon the potential scattering is the resonance scattering which shows the effect of the energy levels of the compound nucleus. Potential and resonance scattering cross sections cannot be added arithmetically, however, but if they involve the same angular momenta, an interference effect occurs. If neutrons of orbital angular momentum l form a compound nucleus in an excited state of spin j, the variation with energy of the cross section for elastic scattering in the neighborhood of this resonance is given by

$$\sigma_{\mathrm{el}}{}^l = 2\pi k^{-2}\frac{2j+1}{2I+1}\left|\frac{\Gamma_n/2}{E-E_r+i(\Gamma_n+\Gamma_a)/2}+e^{i\delta_l}\sin\delta_l\right|^2 + 4\pi k^{-2}\left[2l+1-\frac{2j+1}{2(2I+1)}\right]\sin^2\delta_l. \tag{4}$$

This expression for the scattering cross section has the property of yielding for $\sigma_{\mathrm{el}}{}^l$ a value less than $\sigma^l{}_{\mathrm{Pot}}$ at energies below E_r and a value larger than $\sigma^l{}_{\mathrm{Pot}}$ at energies above E_r. The resulting variation of the cross section with energy is very similar to the variation with wavelength of the optical refractive index in a region of anomalous dispersion. In order to obtain the total cross section for elastic scattering, it is necessary to add to $\sigma_{\mathrm{el}}{}^l$ the nonresonant contributions for all other values of l as given by Eq. (2).

In the present paper, studies using neutrons in the energy range from 20 to 1500 kev will be discussed. For neutrons in this energy range the predominant interaction, especially in the case of light nuclei, is elastic scattering. In almost all cases it is possible to assume that $\Gamma_n \gg \Gamma_a$, i.e., that all other processes may be neglected compared to elastic scattering. Under this assumption, Eq. (4) has the property that the first term vanishes at an energy below E_r and reaches the maximum value

$$\sigma_m = 2\pi k^{-2}(2j+1)/(2I+1) \tag{5}$$

at an energy above E_r. This means that the cross section for elastic scattering should show a variation by σ_m as the neutron energy is varied through the resonance region. The important point is that σ_m is independent of the width of the resonance and also independent of l. Since the wave number of the incident neutron and the spin of the bombarded nucleus are usually known, a measurement of σ_m allows the spin of the compound nucleus j to be determined, provided the energy spread of the neutrons used is appreciably smaller than the width of the resonance.

In many cases it is also possible to determine l, the angular momentum of the incident neutron, which is responsible for the resonance. At energies well below E_r, Eq. (4) goes over into Eq. (2). At the energy at which the first term of Eq. (4) vanishes, the cross section according to

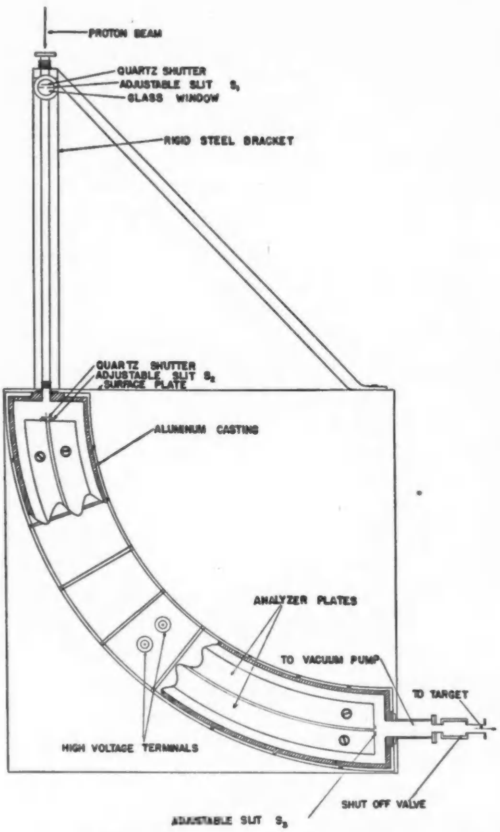

FIG. 2. Electrostatic analyzer which is used for selecting protons of well-defined energy and for controlling the voltage of the electrostatic generator.

Eq. (4) drops from $\sigma^l{}_{\text{Pot}}$ by

$$2\pi k^{-2}[(2j+1)/(2I+1)]\sin^2\delta_l. \qquad (6)$$

If $I, j,$ and k are known, this decrease of the cross section allows a determination of δ_l. From Eq.

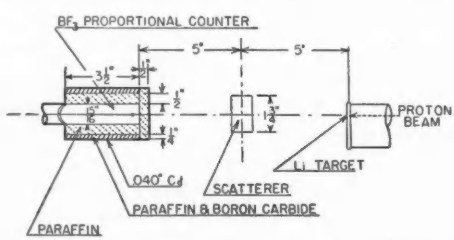

FIG. 3. Apparatus used for measuring total neutron cross sections. The counting rate of the detector on the left is measured with and without the scatterer.

(3) it is then possible to find which value of l is compatible with the experimental results. As was mentioned earlier, Eq. (3) hold only approximately; their use for obtaining a value of l is, therefore, possible only at relatively low neutron energies where the phase shifts for different l differ by large amounts.

Experimental Method

Fast neutrons are obtained from the reaction

$$Li^7 + p \rightarrow Be^7 + n - 1.65 \text{ Mev}. \qquad (7)$$

Protons are accelerated by the electrostatic generator[7] and their energy is measured and regulated by means of an electrostatic analyzer of one meter radius of curvature[8] shown schematically in Fig. 2. The voltage applied to the plates of the analyzer determines the energy of the protons which are able to pass through the analyzer. By adjusting the opening of slits S_1 and S_3, protons of arbitrarily small energy spread

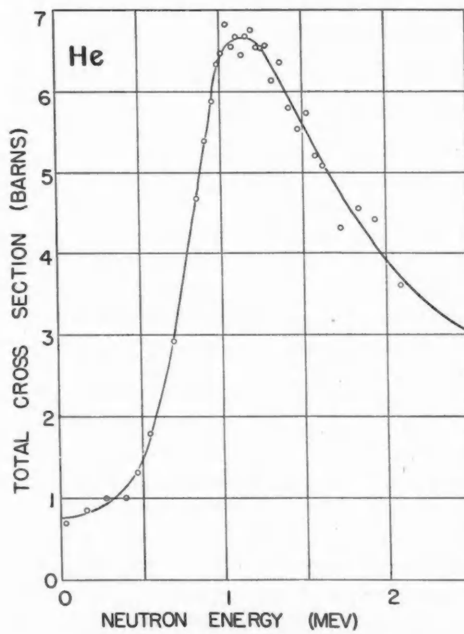

FIG. 4. Total neutron cross section of helium as a function of neutron energy. The effect of an energy level in He^5 manifests itself by the high cross section around 1 Mev.

[7] Herb, Turner, Hudson, and Warren, *Physical Rev.* **58**, 579 (1940).

[8] Warren, Powell, and Herb, *Rev. Sci. Inst.* **18**, 559 (1947).

may be selected. The protons then bombard a thin layer of metallic lithium. Three factors determine the spread in energy of the neutrons which are produced: (1) the spread in energy of the incident protons, (2) the thickness of the lithium target, and (3) the angle subtended by the measuring equipment at the target. This third effect is due to the variation of the energy of the neutrons with the angle between the incident protons and the direction of emission of the neutrons. In principle, the energy spread produced by all three causes can be made as small as one wishes, the only limitation being the Doppler effect which introduces a spread of about 200 ev. In practice, however, the neutron intensity decreases so rapidly with decreasing energy spread that it has not been feasible to improve the resolution in energy below 1 kev.

It was shown recently that the neutrons from the Li(p,n) reaction are not monoenergetic, but that a second group of neutrons of an energy of about 490 kev below the main group is present.[9] These lower energy neutrons, which have an intensity of about 10 percent of the main group, make neutron measurements using the Li(p,n) reaction at energies above 650 kev somewhat unreliable, although in some cases a correction for their effect can be applied.

The measurements to be described in the following section are determinations of total neutron cross sections. The geometry of the apparatus used for most of these experiments is shown in Fig. 3. A layer of the material to be studied is interposed between the neutron source and a sensitive neutron detector. If I_0 represents the counting rate in the absence of the sample, I_S the counting rate with the sample in place, the total cross section is obtained from

$$\sigma_T = N^{-1} \ln(I_0/I_S), \tag{8}$$

where N is the number of nuclei per unit area in the sample. The cross section so calculated has to be corrected for neutrons which reach the detector after having been scattered by the floor

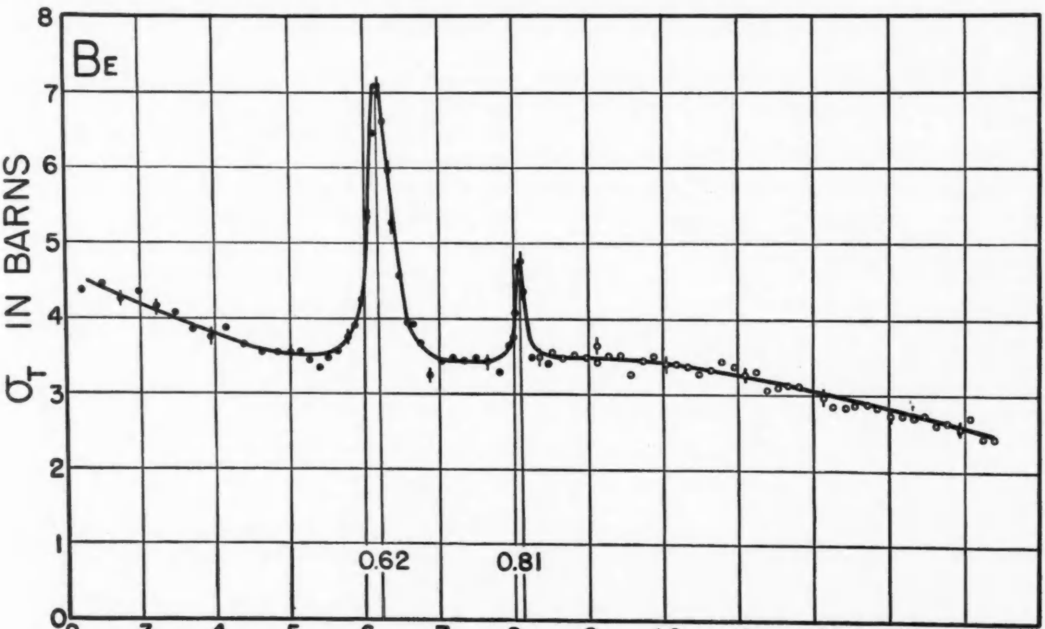

Fig. 5. Total neutron cross section of beryllium.

[9] Johnson, Laubenstein, and Richards, *Physical Rev.* **77**, 413 (1950).

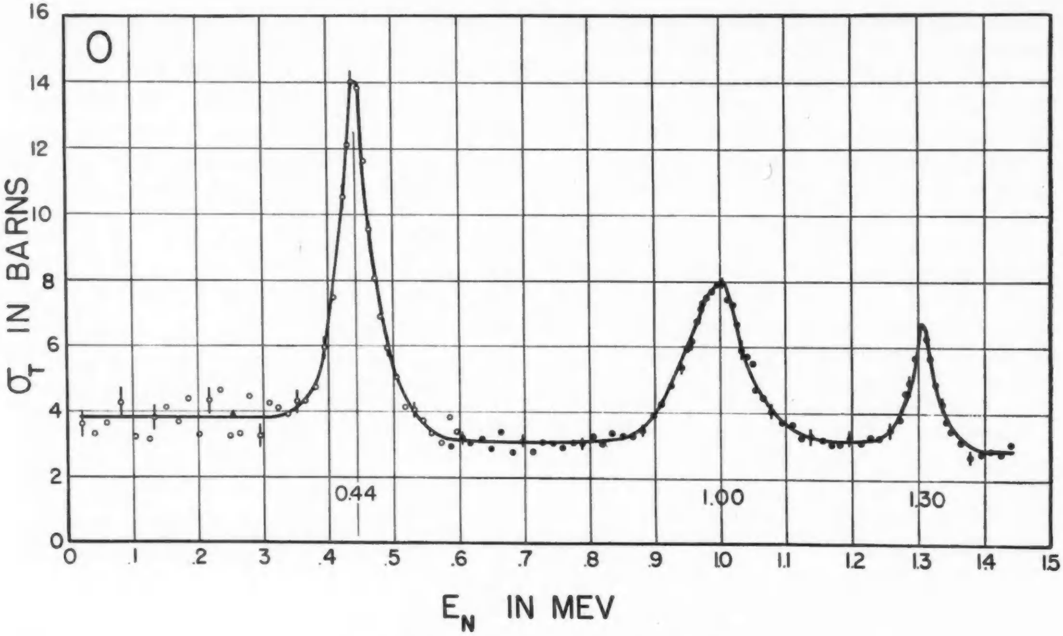

FIG. 6. Total neutron cross section of oxygen.

or other surrounding materials, and for neutrons scattered by the sample into the detector.

Results of Total Cross Section Measurements

The lightest element on which measurements have been carried out at Wisconsin is helium. Staub and Stephens[10] discovered an excited state in He^5 formed when He^4 was bombarded by 1-Mev neutrons, but subsequent work[11] raised some doubt as to whether this level was single or double. The results shown in Fig. 4[12] show no evidence for a double peak although the energy resolution used was at least as good as that of previous experiments. If one wishes to interpret the results in terms of the Breit-Wigner formula, one has to use Eq. (4) in a modified form because of the very large width of the resonance. Calculations by R. K. Adair indicate that the experimental data can be fitted best by assuming that neutrons of one unit of angular momentum (p-neutrons) form a compound nucleus of spin $\frac{3}{2}$.

Another light element for which measurements were carried out is beryllium.[13] Figure 5 shows the total cross section of beryllium as a function of neutron energy. Resonances are observed at 625 and 810 kev. By applying Eq. (5)

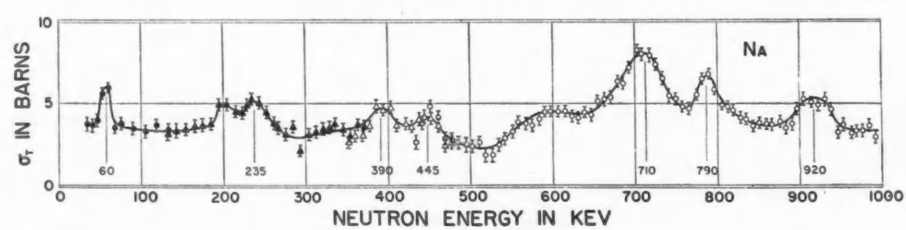

FIG. 7. Total neutron cross section of sodium.

[10] H. Staub and W. E. Stephens, *Physical Rev.* **55**, 131 (1939).
[11] H. Staub and H. Tatel, *Physical Rev.* **58**, 820 (1940).
[12] Bashkin, Petree, Mooring, and Peterson, *Physical Rev.* **77**, 748 (1950).
[13] C. K. Bockelman, *Physical Rev.* (to be published).

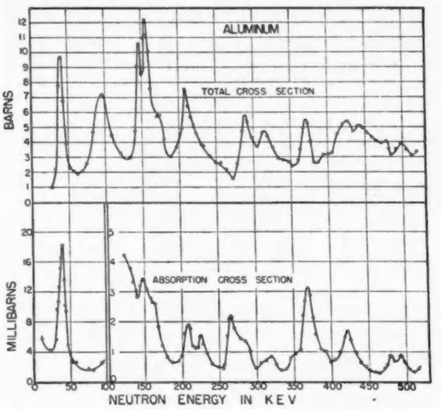

FIG. 8. Upper part: Total cross section of aluminum. Lower part: Cross section for the radiative capture of neutrons by aluminum as measured by the radioactivity of Al^{28}.

to the 625-kev resonance the value obtained for j was 3. Assuming that Be^9 has a spin $\frac{3}{2}$, neutrons of at least one unit of orbital angular momentum are required to form a compound nucleus of spin 3. The method outlined previously for obtaining the l-value cannot be used here, since at this energy $\sigma^0{}_{Pot}$ is very large compared to the cross section for potential scattering of higher angular momentum. It is to be expected, however, that the widths of resonances in this energy region would decrease very rapidly with increasing l. The observed rather large width of 35 kev would, therefore, favor an interpretation in terms of as low an orbital angular momentum as is compatible with the measured value of σ_m, i.e., one unit.

Measurements on oxygen are shown in Fig. 6.[13] The three levels at neutron energies of 440, 1000, and 1300 kev have also been found by the Minnesota group.[14] Applying again Eq. (5), all three levels have values of σ_m which can be fitted best by assuming that the compound nucleus formed has a spin $j=\frac{3}{2}$. Since the spin of O^{16} is zero, $j=\frac{3}{2}$ can be obtained from either p- or d-neutrons. On the basis of the widths of the levels, one will again be inclined to favor the lower l-value.

As one goes from O^{16} to Na^{23}, one finds that the number of levels in each nucleus increases

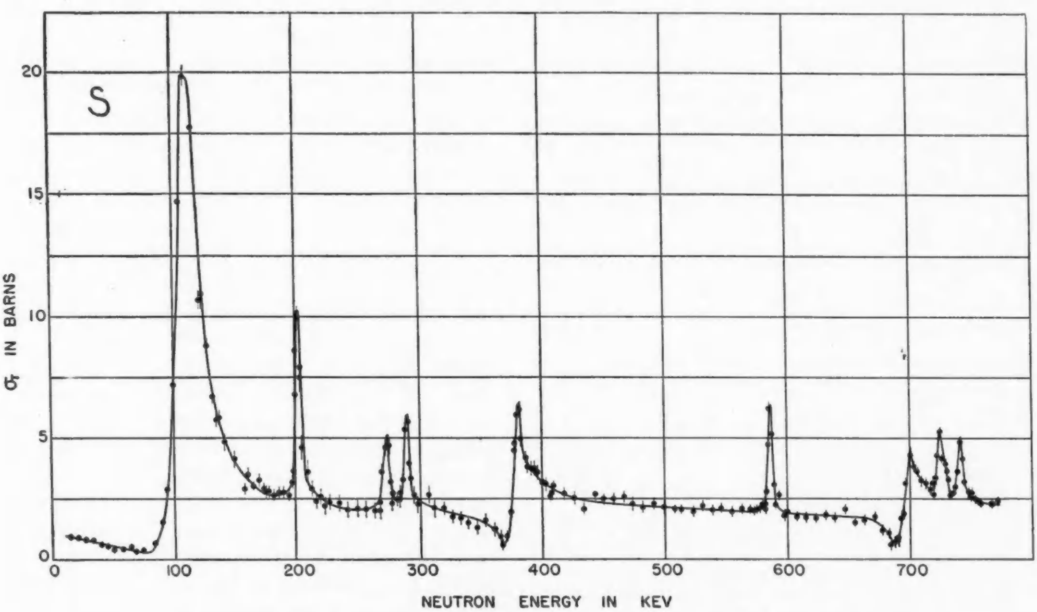

FIG. 9. Total neutron cross section of sulfur showing the effect of three s-levels and six levels of higher angular momentum.

[14] Freier, Fulk, Lampi, and Williams, *Physical Rev.* **78**, 508 (1950).

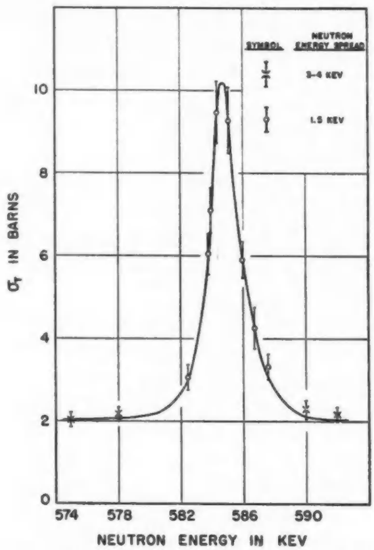

FIG. 10. Total neutron cross section of sulfur at neutron energies around 585 kev measured with an energy resolution of about 1.5 kev.

rapidly and that their widths decrease. It is apparent from Fig. 7[15] that, with the neutron energy spread of 20 kev used in these measurements, the Na levels are not completely resolved. A comparison with Eq. (5) indicates that the amplitude of several of the observed peaks is less than that expected for the lowest possible value of j.

In order to study elements heavier than sodium, it becomes, therefore, desirable to improve the energy resolution. Measurements of the cross section of Al^{27}, carried out with a neutron energy spread of less than 8 kev, are shown in the upper half of Fig. 8. Even this resolution does not appear sufficient to separate the levels completely.

On the basis of most models of the nucleus which have been proposed it would be expected that the density of levels should increase rapidly with the number of nucleons in the nucleus and with the excitation energy of the compound nucleus. As a consequence, it was surprising that it was possible to resolve the levels in sulfur, since sulfur not only is heavier than aluminum but, in addition, the compound nucleus is formed at an excitation energy 1.3 Mev higher than in aluminum. Figure 9 gives the cross section of S measured with a resolution of about 9 kev in the flat portion, and with a resolution of 3 kev near the peaks.[16] Nine energy levels are observed for neutron energies below 775 kev. In this energy range, resonances due to s-neutrons can be distinguished easily from those formed by neutrons of higher angular momentum by the dips preceding the peaks as given by Eq. (6), since practically all the potential scattering is s-scattering. Therefore, the peaks appearing at 110, 382, and 700 kev can be identified as s-levels, while the remaining levels must be formed by neutrons of higher angular momenta. Since the spin of S^{32} is zero, the second term of Eq. (4) vanishes for $l=0$, and there should be some energy below E_r at which $\sigma_{el}{}^0$ should go to zero. That the observations give finite cross sections at all energies is due to potential scattering of higher angular

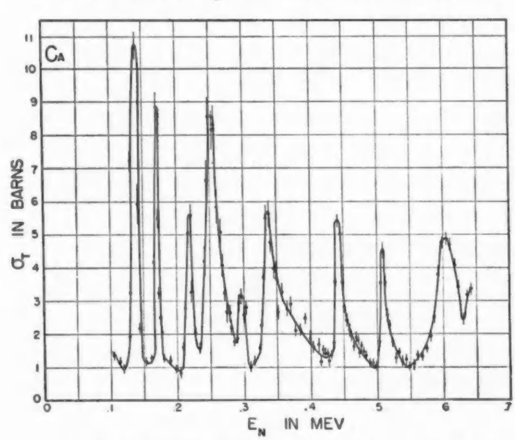

FIG. 11. Total neutron cross section of calcium.

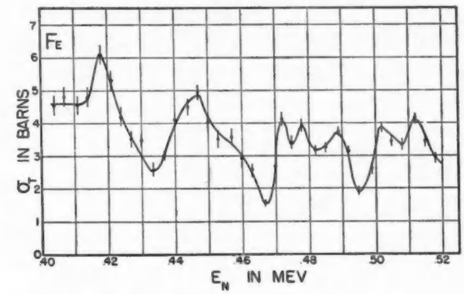

FIG. 12. Total neutron cross section of iron.

[15] Adair, Barschall, Bockelman, and Sala, *Physical Rev.* **75**, 1124 (1949).

[16] Peterson, Barschall, and Bockelman, *Physical Rev.* **79**, 593 (1950).

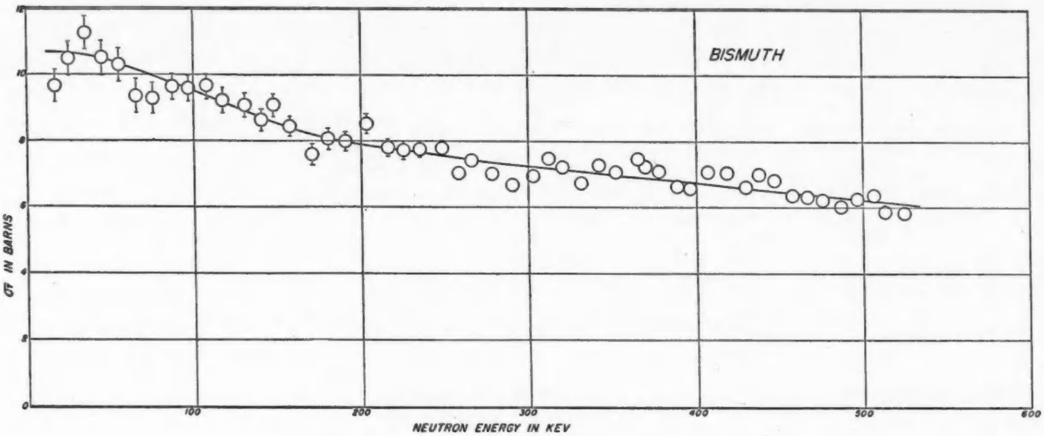

FIG. 13. Total neutron cross section of bismuth.

momenta, to the presence of the sulfur isotopes S^{33} and S^{34}, and to the fact that the assumption $\Gamma_n \gg \Gamma_a$ may not be quite justified. It may be noted that the *s*-resonances are appreciably wider than the other peaks, as would be expected on theoretical grounds. The value of σ_m agrees for the three *s*-levels with that predicted by Eq. (5), while the energy resolution is not sufficient to obtain reliable values for the levels of higher angular momentum. Figure 10 gives the results of a measurement on the peak at 585 kev with a resolution of about 1.5 kev. The measured width has now become greater than the energy spread of the neutrons used so that one can be reasonably sure that the peak has been resolved. From the observed value of σ_m one obtains for this level $j=\frac{3}{2}$.

From the study of atomic structure it is well known that groups of atomic electrons form closed shells. There is good evidence for the existence of closed neutron and proton shells in nuclei also. Nuclei containing 20, 50, 82, or 126 neutrons or protons appear to be particularly stable.[17] If a neutron is added to a nucleus containing only closed shells, the neutron might show little interaction with the other nucleons. A study of the scattering of neutrons by a closed shell nucleus might be expected, therefore, to show a behavior similar to that of light nuclei, i.e., wide levels with wide separation. Since Ca^{40}, consisting of 20 neutrons and 20 protons, has both closed neutron and closed proton shells, its neutron cross section was measured. The results given in Fig. 11 do not indicate a markedly wider level spacing than is observed for other elements of comparable weight. The only unusual feature is the very small cross section between peaks showing values appreciably smaller than would be expected on the basis of Eq. (2).

As an example of a heavier element in which levels were found, Fig. 12 gives a measurement on Fe over a small energy range.[18] From the fact that the cross section at the peaks does not reach the values predicted by Eq. (5), it may be concluded that the resolution of 4 kev used in this experiment was insufficient to resolve the levels completely. Since measurements using slow neutrons indicate a level spacing of only a few volts in elements of mass number around 100, it did

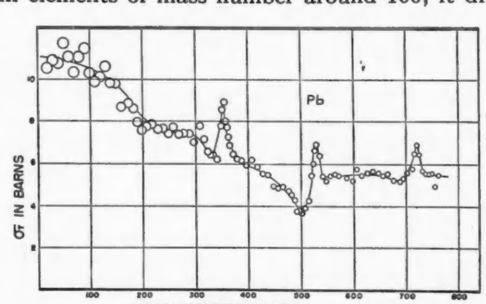

FIG. 14. Total neutron cross section of common lead measured with a neutron energy spread of 10 kev.

[17] M. G. Mayer, *Physical Rev.* **74**, 235 (1948).

[18] F. Ajzenberg, private communication.

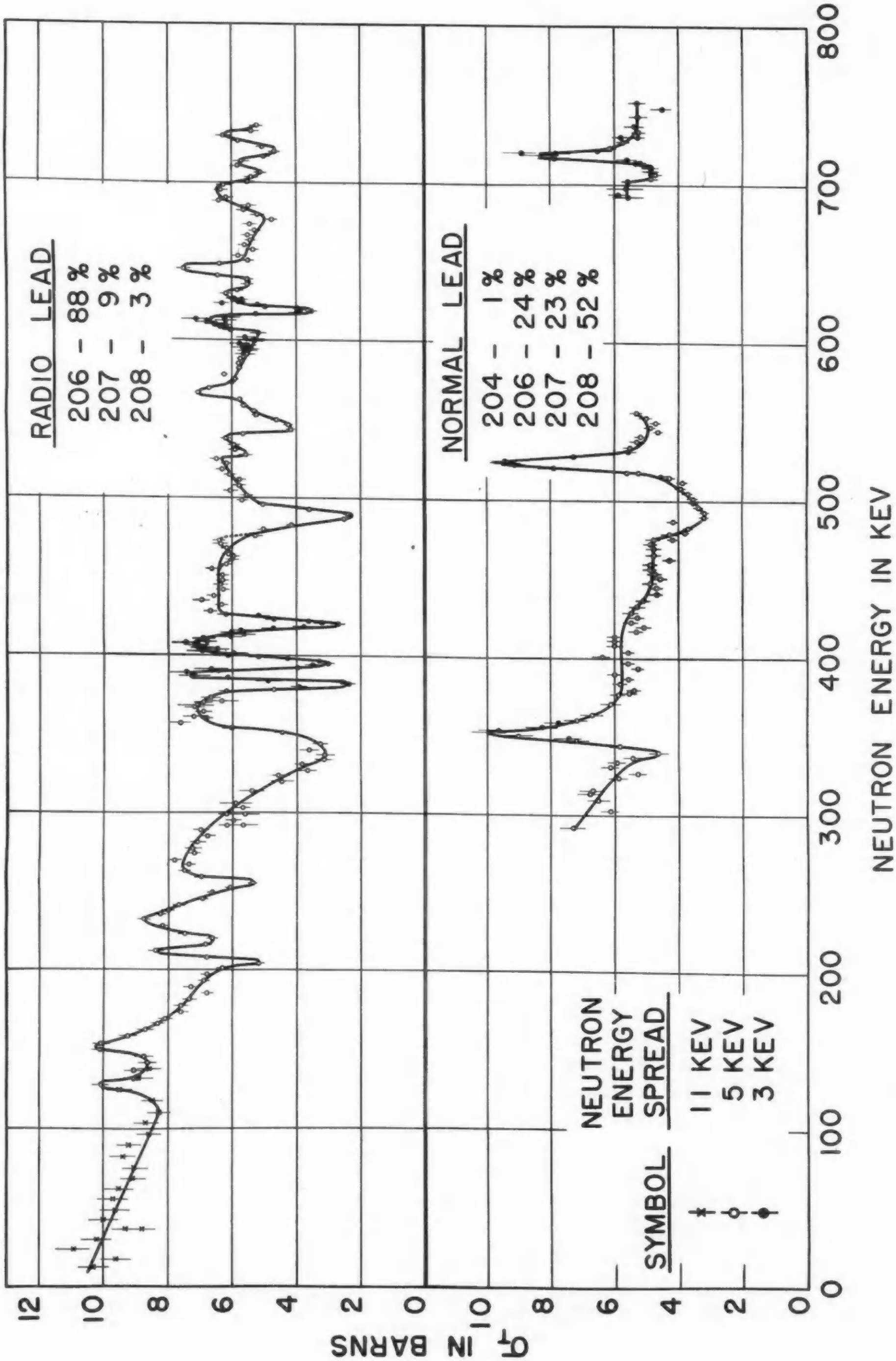

FIG. 15. Total neutron cross section of radiogenic lead (upper part) and of common lead (lower part).

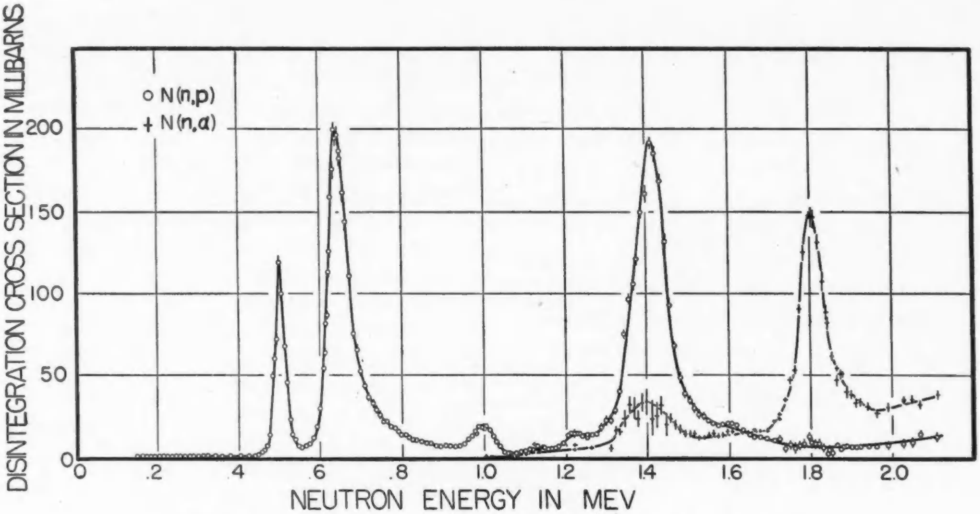

FIG. 16. Cross section for the disintegration of nitrogen by fast neutrons.

not appear likely that levels could be found in elements much heavier than iron with the energy resolution of about 1 kev available for fast neutrons. Exceptions might be lead and bismuth in which slow neutron experiments have not revealed any levels and which are closed shell nuclei. With a resolution of 10 kev, no levels were found in Bi (Fig. 13) with fast neutrons.[19] In Pb, however, three peaks were observed as shown in Fig. 14. These peaks were studied further with a resolution of 3 kev, and the results are shown in the lower part of Fig. 15.[20] An interpretation of these levels is complicated by the fact that common Pb has three abundant stable isotopes: Pb^{206}, 24 percent; Pb^{207}, 23 percent; Pb^{208}, 52 percent. The observed values of σ_m are so large, however, that only the assumption that the most abundant isotope, Pb^{208}, is responsible for the peaks leads to a reasonable interpretation. As a check on the isotopic assignment, the total cross section of a sample of radiogenic lead, which consisted mostly of Pb^{206}, was measured. The results shown in the upper part of Fig. 15 indicate that Pb^{206} is not responsible for the levels observed in common lead.[20] But the fact that many levels could be resolved in the compound nucleus Pb^{207} came as a surprise, since this nucleus does not have a closed neutron shell.

[19] Barschall, Bockelman, Peterson, and Adair, *Physical Rev.* **76**, 1146 (1949).
[20] Peterson, Adair, and Barschall, *Physical Rev.* **79**, 935 (1950).

Results of Absorption Measurements

While for neutrons of some hundred kev energy elastic scattering is the most important interaction, energy levels can also be studied by observing the absorption of the neutrons even though the absorption will have no measurable effect on the total cross section. Such measurements are of particular interest, if the results can be compared with the information about the same levels deduced from scattering experiments.

Some results are available for two types of absorption phenomena. One possibility is that the compound nucleus will disintegrate by emission of a charged particle, such as a proton or alpha-particle, another that the compound nucleus will go to its ground state by radiation of a gamma-ray; the latter process is usually referred to as *radiative capture*.

As an example of the first kind, the reaction

$$N^{14}+n\rightarrow C^{14}+p+0.6\text{ Mev} \tag{9}$$

was investigated.[21] The disintegration protons were observed in an ionization chamber filled with nitrogen. From the number of protons the

[21] C. H. Johnson and H. H. Barschall, *Physical Rev.* (to be published).

disintegration cross section could be determined. The results of the measurements are shown in Fig. 16. At least four energy levels were found in the compound nucleus N^{15}. Evidence was also obtained that for the alternative reaction

$$N^{14}+n \rightarrow B^{11}+\alpha-0.3 \text{ Mev}, \quad (10)$$

a resonance occurs at the same energy as one of the resonances for the (n,p) reaction.

As an example of an (n,γ) reaction, the reaction $Al^{27}+n \rightarrow Al^{28}+\gamma$ was investigated. The Al^{28} nucleus is radioactive and decays with the emission of beta-rays to Si^{28} with a half-life of 2.4 minutes. By measuring the activity of Al^{28} it is possible to find the cross section for the (n,γ) process. The lower part of Fig. 8 shows the cross section of aluminum for radiative capture of fast neutrons.[22] One would expect to find the same energy levels in this measurement as those shown in Fig. 8 for the total cross section of Al. The agreement is fairly good, but the lack of resolution of the levels makes the correlation somewhat uncertain.

[22] R. L. Henkel and H. H. Barschall, *Physical Rev.* **80**, 145 (1950).

A Demonstration of Specific Acoustic Resistance

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THIS paper describes an experiment that demonstrates the part which specific acoustic resistance may play in the behavior of a mechanical system under impulsive loading.

Impulsive loading occurs when a mechanical system is struck a sharp blow. A simple case is the striking of a nail with a hammer; a complex one, the landing of an airplane. In both cases, the immediate effect of the impact is essentially the same. An impulse or transient stress wave is induced into the system. The impulse may then affect the system in a variety of ways. Indeed, in many practical cases the system is so complex that its reaction cannot be treated simply. However, any analysis necessarily involves a consideration of how the impulse is propagated through, and distributes itself within the system. The apparatus and experiments described here can be used to considerable advantage in demonstrating how an abrupt change in specific acoustic resistance within the system can affect the transmission of the original shock.

The experiment is relatively simple. Basically, the apparatus, shown schematically in Fig. 1, consists of an impulse inducer, a transmitter and a receiver. It functions as follows. A steel ball is allowed to fall or roll from a preselected height in such a way that it strikes a steel rod (the left rod in Fig. 1). The impact sets up an elastic wave which travels along the rod. On reaching the end of the rod, part of the wave enters a second rod (right rod in Fig. 1) which is in contact with the first rod. However, if the interface between the two rods will support no tension, *viz.*, if they are simply butted against one another, that part of the wave which enters the second rod becomes trapped in it and the second rod flies off. By suspending this second rod as a ballistic pendulum its acquired momentum can be readily determined.[1]

The fractional part of the total impulse imparted to the system by the ball, which is transmitted to the second rod, will be governed by the relative specific acoustic resistances of the two rods. This is true provided that the two rods have the same cross-sectional areas and, also, that the time required for the impulse to travel a distance equal to twice the length of the second rod is greater than the total duration of the impulse. The first condition can be readily met. The second requires that the product, twice the diameter of the ball times the velocity of sound in it, be less than the product, twice the length

[1] The behavior of such systems, wherein both rods were of the same material, has been extensively studied by Hopkinson and his co-workers. The reader who is interested in pursuing experiments of this type will find their papers extremely illuminating, particularly J. E. Sears, *Proc. Camb. Phil. Soc.* **14**, 257 (1907), and B. Hopkinson, *Proc. Roy. Soc. London* **A213**, 437 (1914).

of the second rod times the velocity of sound in it. The latter requirement will be satisfied if the length of the first rod is greater than the diameter of the ball, since the ball will remain in contact with the rod during the time which it takes an impulse to travel across the ball and back again.[2] In the case where both rods are of like material, and thus perfectly matched acoustically, the second rod will acquire all of the momentum, since none will be reflected at the interface. The first rod will remain at rest. If, however, the two rods are of different materials and hence have different specific acoustic resistances, part of the impulse will be transmitted into the second rod and part will be reflected back into the first. More precisely, it can be shown[3] that in terms of the momentum M_I striking the interface between the rods, the reflected and transmitted momentums M_R and M_T, respectively, are given by

$$M_R = M_I \frac{R_2 - R_1}{R_2 + R_1}; \quad M_T = M_I \frac{2R_2}{R_2 + R_1}, \qquad (1)$$

where R_1 and R_2 are the respective specific acoustic resistances[4] of the first and the second rods.

The demonstration then proceeds as follows. A steel rod is placed in the pendulum. The ball is allowed to roll down the inclined plane. It strikes the first rod, which is also steel, and the second rod flies off. The deflection of the pendulum is noted and the momentum of the second rod calculated. The experiment is then repeated with other metal rods substituted for the second rod. The first rod is not changed. The respective momentums acquired by the different rods, when tabulated, will show that a steel rod acquires the most momentum, a brass somewhat less and a 24*ST* dural the least. Demonstration of this fact is the primary purpose of the test. The way in which the momentum of a shock will distribute itself through a mechanical system which has been impulsively loaded can be affected by abrupt changes in specific acoustic resistance within the system.

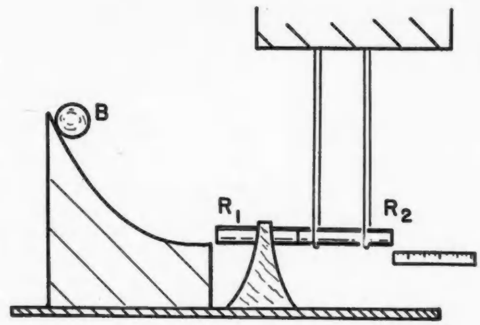

FIG. 1. A schematic drawing of experimental setup. Ball B is impulse inducer (steel ball); R_1, transmitter (steel rod); R_2, receiver (rod being tested).

Qualitatively, this effect can be observed by holding the first rod by hand instead of clamping it during the experiment. With the second rod steel one can hardly feel the impact of the ball. However, with the second rod dural, one can feel the first rod experience a noticeable jump.

The experiment has been tried using a 1-inch diameter steel ball, rolled from a height of about 5 inches,[5] and ½-inch diameter rods. The first rod was steel, about two inches long. The pendulum consisted of two loops of thread in which the second rod was laid. The threads were about a foot and a half long. Usually the deflection of the pendulum was a few centimeters. Results obtained in one series of trials are listed in Table I.

It is assumed in performing the experiment that (1) successive impacts of the ball produce identical impulses and (2) the contact between the ends of the two rods is good. The former will be substantially the case if considerable care is used in releasing the ball. Good surface contact

TABLE I. Summary of momentum data.

Rod material	Length of rod (cm)	Mass of rod (g)	Average deflection of pendulum (cm)	Velocity of rod (cm/sec)	Momentum of rod (g cm/sec)
Steel	6.7	65.8	4.4	20	1300
Brass	7.1	75.8	3.2	15	1100
24*ST* dural	6.9	24.5	6.8	31	760

[2] The first paper of Reference 1 describes investigations in which such times were verified experimentally.

[3] The boundary conditions are (a) continuity of pressure and (b) continuity of particle velocity. These must hold for every point on the incident wave. See for example R. H. Cole, *Underwater Explosions* (Princeton University Press, 1948), p. 52.

[4] Specific acoustic resistance as used here is defined as the product, density times velocity of sound.

[5] An equally good scheme would be to suspend the ball as a pendulum.

TABLE II. Comparison of accepted and measured values of specific acoustic resistance.

Rod material	Specific acoustic resistance (g/sec cm²) ×10⁴ Accepted	Measured
Brass	300	280
24*ST*	130	160

is best assured by carefully grinding the surfaces and using extreme care in aligning the two rods.

If desired, the calculations can be carried further to find specific acoustic resistances of the materials. Equation (1) is used for this purpose. The momentum trapped in the aluminum or brass rod is compared with that trapped in the steel rod. The acoustic resistance of the latter is assumed known and equal to that of the steel rod used to transmit the impulse.

It follows from Eq. (1) that the specific acoustic resistance of the unknown rod will be given by

$$R_U = \frac{R_S}{2(M_S/M_U)-1}, \tag{2}$$

where M_S is the momentum of the steel rod and M_U that of the unknown. Since the displacement of the pendulum is nearly proportional to its velocity the formula is more conveniently written

$$R_U = \frac{R_S}{2(m_S x_S/m_U x_U)-1}, \tag{3}$$

where m_S and m_U are the mass and deflection, respectively, of the steel rod and x_S and x_U are those of the unknown.

Values of specific acoustic resistance calculated in this way from the data in Table I are listed in Table II. The specific acoustic resistance of the steel rod was taken equal to 390×10^4 g/sec cm² in making the calculations. Yet the agreement between commonly accepted values and the experimental results is good enough to justify the calculation. With more carefully controlled conditions than were used in the tests described here, the precision could be increased considerably.

The demonstration described here is just one of many similar experiments which will undoubtedly occur to the reader. For example, the partition of momentum between two parts of a system can readily be demonstrated by using two ballistic pendulums. The simplicity of the apparatus places the emphasis on the basic concepts involved in the reactions.

The common world in which we believe ourselves to live is a construction, partly scientific, partly pre-scientific. We perceive tables as circular or rectangular, in spite of the fact that a painter, to reproduce their appearance, has to paint ellipses or nonrectangular quadrilaterals. We see a person as of about the same size whether he is two feet from us or twelve. Until our attention is drawn to the facts, we are quite unconscious of the corrections that experience has led us to make in in interpreting sensible appearances. There is a long journey from the child who draws two eyes in a profile to the physicist who talks of electrons and protons, but throughout this journey there is one constant purpose: to eliminate the subjectivity of sensation, and substitute a kind of knowledge which can be the same for all percipients. Gradually the difference between what is sensed and what is believed to be objective grows greater; the child's profile with two eyes is still very like what is seen, but the electrons and protons have only a remote resemblance of logical structure. The electrons and protons, however, have the merit that they may be what actually exists where there are no sense-organs, whereas our immediate visual data, owing to their subjectivity, are almost certainly not what takes place in the physical objects that we are said to see.—BERTRAND RUSSELL, Human Knowledge, 1948.

Preparation of College Physics Teachers at the Pennsylvania State College*

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FOR a long time the physics department of The Pennsylvania State College has been acutely conscious of a great and pressing need for giving prospective college teachers more adequate professional training. About two years ago it was decided to do something about it.

The training program which was started on a deliberately conservative, experimental basis has developed thus far along three lines. First, there were instituted so-called professional colloquia; second, preliminary steps were taken looking toward the establishing of teaching apprenticeships; third, there was experimentation with two seminars which it is hoped will be of value especially to future teachers.

Our department, like most others, operates a weekly colloquium. This has been the one occasion during the week when our whole physics family, teaching staff and graduate students, could be together to talk about advances in physics or about their own research. The attendance has averaged between ninety and a hundred. It occurred to us that it might not be inappropriate if such weekly gatherings occasionally gave consideration to the professional interests of physicists, aside from research.

Since then we have devoted one colloquium each month to professional matters. While not all of these have dealt with teaching directly, they have all been definitely helpful in acquainting future teachers with what they should take into account when training physicists for various types of jobs, and with the organizational aspects of the physics profession.

The following partial list of speakers and their topics will illustrate what we are trying to do in this direction. K. K. DARROW, of the Bell Telephone Laboratories, spoke on "The American Physical Society, Its Organization, Role and History"; H. A. BARTON, Director of The American Institute of Physics, on "The American Institute of Physics"; M. H. TRYTTEN, of The National Research Council, on "Physics as Seen from Washington"; ELMER HUTCHISSON, Editor of the *Journal of Applied Physics*, on "Problems of a Physics Editor." Our own MARSH W. WHITE discussed "Physicists in War and Peace"; GLENN GIDDINGS, of the General Electric Company, spoke on "Physicists in Industry"; and R. D. BENNETT, of the Naval Ordnance Laboratory, on "Physicists in Government Employ." To emphasize the need for broad thinking about physics, we secured HENRY MARGENAU, of Yale University, and W. F. G. SWANN, of The Bartol Research Foundation, to discuss some of its philosophical aspects, and HAROLD C. UREY, University of Chicago, some of its social implications in an atomic age.

Interspersed among these have been colloquia dealing directly with teaching. Thus I talked on "What is a College Professor?" There were talks by three distinguished psychologists, C. R. CARPENTER, of our campus, speaking on "What a Professor of Physics Should Know about Psychology" and on the "Psychology of Visual Aids," R. G. BERNREUTER, also of The Pennsylvania State College, on "What a Professor of Physics Should Know about Student Advising," and C. E. BUXTON, of Yale University, describing his work in "Training Teachers in The Graduate School."[1] LEO NEDELSKY, of the University of Chicago, lectured on "Testing in Terms of Specific Teaching Objectives";[2] RICHARD M. SUTTON, on "The Art of Lecture Demonstration." Our own D. C. DUNCAN talked on "Characteristics of a Good General Physics Teacher." A. O. MORSE, of our campus, Assistant to the President in Charge of Resident Instruction, talked on "General Education, Its Meaning and Significance." DUANE ROLLER addressed us on "The Basic Role of Physics in a Liberal Education."

Frequently when there is a visiting speaker the afternoon colloquium session is followed in the evening by a round-table discussion in which

* Invited paper presented before the American Association of Physics Teachers, New York Meeting, February 1950, as part of a Panel Discussion: Methods of Training College Teachers of Physics.

[1] C. E. Buxton, *Am. J. Physics* **17**, 571 (1949).
[2] See also Leo Nedelsky, *Am. J. Physics* **17**, 345 (1949).

graduate students may become better acquainted with the speaker and may discuss his subject with him at closer range.

All this has created much interest and a considerable demand for somewhat more formal teacher preparation. Hence, we decided last year to try several other things. First, we took steps gradually to convert our graduate teaching assistantships more definitely into teaching apprenticeship opportunities.

To this end we carried out a more carefully planned and administered orientation program than previously for new junior staff members, including assistants. All staff members, junior and senior, were asked to be on hand one week before the beginning of the semester for a series of meetings and conferences designed to offer opportunities for becoming thoroughly acquainted with department and College policies, procedures, and standards; with the purposes, scope and content of elementary courses; with facilities and equipment of the department; and so on. This program was planned and directed by Marsh W. White and Robert L. Weber.

We then determined to follow through with more careful coordination of the teaching of junior staff members. Conferences of teaching staffs of the various physics courses are now held periodically. Often these are much more than discussions of what to do next, or of what shall be asked in examinations. Some of them get into the serious consideration of basic teaching objectives, of the learning process, and of fundamental methodology.

It should be emphasized that we do not claim any originality for the orientation and coordination parts of our program. We realize that many departments have systematically coordinated and directed the work of graduate teaching assistants quite effectively for years. However, we hope that in our own situation we shall succeed in greatly improving and broadening these efforts in such a way as eventually to make them significant and integral parts of a systematic apprenticeship program for future teachers.

Our next move was to experiment with class visitation and observation of teaching. We had done some of this in the past in laboratory classes. Last year we tried it with a few beginning teachers who were instructing so-called recitation or discussion sections. We started with those who took the initiative in asking for such help, who anticipated benefits from it, who did not feel that the supervisor was "snooping." Preliminary results were encouraging. We hope in this way to learn how to do such classroom visitation effectively without the alleged harmful effects upon teacher and class which so many college teachers seem to fear. I feel confident that apprentice teachers will find such training very helpful.

Another modest educational experiment last year was the introduction at the graduate level of a seminar on scientific methodology. This was taught by our Richard C. Raymond. We do not think of this course as having a particularly philosophic bias or orientation, though naturally we do want it to be sound and respectable from a philosophic point of view. Rather we consider it as definitely a physics course, a course among physicists thinking and talking critically about their science—and, of course, related sciences—using the vocabulary and idiom of practicing physicists rather than of professional philosophers.[3] We very much hope that this seminar will turn out to be especially valuable to future physics teachers in helping them to understand and interpret their science more intelligently and clearly.

Next, I wish to report on an experiment with a seminar on college teaching. This was my direct responsibility. Participation was on a purely voluntary basis. Some twenty graduate students and junior staff members enrolled. Some dropped out, for various reasons, so that about half that number remained at the end. The class met every other week for an hour and a half in the evening.

The purpose of this seminar was rather broad. It was predicated on two basic assumptions whose validity not everybody will grant, but which I consider both valid and important. They are:

First, a physics teacher must be more than a

[3] This does not mean that we have objections to courses taught by philosophers. We rather encourage prospective teachers to study in the department of philosophy. However, we feel that something is to be gained also from studying the philosophy of science in the work-a-day language of scientists as well as that of philosophers. There does seem to be a difference.

physicist. Knowing physics does not itself qualify a man to teach physics. While mastery of subject matter is indispensable for success in teaching, it is not sufficient. Certain personality and character traits are necessary; as are also a thorough acquaintance with and a ready command of the "tricks of the trade." The physics teacher should be a professional in the classroom, not an amateur.

The second assumption is that being a physics teacher involves even more than being an expert classroom teacher. Teachers have important obligations outside the classroom.

Therefore, the objectives of the seminar were announced as follows:

1. To introduce graduate students in physics who are looking forward to college teaching as a career to the more important contemporary problems of higher education—with special reference to possible contributions physics teachers can make to their solution.

2. To consider the various functions and obligations of a college teacher in regard to (a) classroom teaching; (b) student counselling; (c) participation in curriculum-building and other institutional affairs; (d) scholarship, research, and other creative work; (e) community life.

It would not seem unreasonable to expect a young man planning to go into college teaching to give some serious thought to the variety of duties he will encounter later. Why should he not learn something, for instance, about the learning process, about the psychology of youth, and how to advise and guide them? Why should he be allowed to tinker with young minds if he has never bothered to inform himself on how human minds function? And why should he not become informed on educational matters of broad institutional concern even though they may not be directly related to physics? Why shouldn't he have at least a modicum of such professional knowledge and awareness of issues before he takes a job?

3. To study effective procedures and techniques for the formulation and realization of teaching objectives. This includes consideration of (a) how to formulate curricular and course objectives, (b) the value and methodology of the lecture, (c) the recitation or discussion period, and the laboratory, (d) diagnostic and remedial devices, (e) teaching "how to study," and so on.

It is the third objective which absorbed most of the time and effort of the seminar.

4. To develop a professional attitude and consciousness on the part of the prospective college teacher by considering (a) problems of the teaching profession as such, (b) professional organizations, their functions and activities, (c) professional literature, (d) Federal and private agencies promoting the study and improvement of teaching, (e) legal problems, and (f) professional ethics.

It is amazing how completely illiterate and ignorant most graduate students and beginning college teachers are about our profession as a profession; how blissfully unaware they are, for instance, of the existence of a literature of teaching and of higher education. To be convinced of this, one need only to ask a typical group of graduate students of physics expecting to go into college teaching to make a list of journals or books devoted to teaching, or of professional organizations and agencies devoted to fostering the interests of higher education, or, say, a list of leaders in American education, or a list of American colleges or universities carrying on important education experiments. The lists produced will be notable for neither significance nor completeness. In my opinion, this should not be so because it means that these students do not know what is going on in the educational world and therefore have no perspective or background against which to develop or orient their own educational philosophy or practice.

Over and over again have my students and I been driven to ask two questions when we were confronted with a problem for which we had no solution: first, what is in the literature about this problem, i.e., what is present knowledge, practice and educational theory regarding it?, and second, if we do not know the answer and cannot find it in the literature, how could we go about finding the answer experimentally? The habit of resorting to experimentation for the solutions to

educational problems, and of searching the literature before planning such experimentation, seems to be virtually nonexistent among college professors. By and large they discuss, and vote upon, issues mainly on the basis of private opinion. Young teachers whose habits and attitudes have not yet petrified should be encouraged to use more scientific and professional bases for decisions and actions in things educational.

This seminar has not yet been as successful as it should have been nor will it be for some time to come. We shall have to learn a great deal more about how to conduct such a seminar, and, indeed, much more about the professional aspects of teaching, before it can be what it was intended to be. Nevertheless, it is my belief that a good start was made and that in time such a seminar can become exceedingly valuable as a device for the preparation of college teachers of physics.

There is a phase of teacher preparation, which for us locally is still a matter for future action, namely, the seeking of more extensive cooperation of other departments, especially the departments of education and psychology. A well-balanced training program might well include courses on the learning process, and on student counselling. In my opinion such courses should be designed specifically for mature graduate students intending to go into college teaching and should prescribe no prerequisites of earlier work in those fields. As I see it now, I would not want such courses to exceed in credit value a total of more than, say, six semester hours, including the kind of work represented by our physics department's teaching seminar. Allowing that amount in the three or four years of graduate work of a future college teacher would not, in my opinion, lower standards for the Ph.D. Frankly, I hope we can persuade other departments to offer such courses and that our apprentice teachers will want to take them.

In conclusion, let me say a few words by way of self-defense! Every doughnut is, for better or for worse, accompanied by a hole. Some people, while meditating or philosophizing on doughnuts, concentrate on the doughnut itself, while others dwell on the hole. This paper is in line with the former. It looks at the doughnut. No doubt many readers will be inclined instead to point out the hole. Hence the need for this self-defense.

The writer did *not* say that a person cannot become a superior teacher without formal professional training. He does say that a judicious amount of it would help a young teacher to avoid many pitfalls and to achieve maximum effectiveness much sooner than if he had no such training.

He disowns any suggestion that we should now *require* such professional training. He does consider it possible that some day such training courses and apprenticeship opportunities will be sufficiently adequate and effective to justify requiring them. It is doubtful that this will happen in the immediate future.

He has not tried to settle the age-old argument as to whether teaching is an art or a science. He did say that prospective teachers should be habituated in the use of at least two scientific methods: searching the pertinent literature, and engaging in experimentation relative to teaching problems.

He denies having said that the business of training college teachers of physics should be turned over to "educationists." He does believe that departments of education and psychology can make a contribution to such training.

He refuses to admit having said, or intimated, or even lightly hinted that we now teach the prospective physics teacher too much physics.

He disclaims any assertion, or even unexpressed thought, that we should not require research dissertations of the Ph.D. candidate who proposes to go into teaching.

He most certainly did not assert that the training program at the Pennsylvania State College is anywhere near perfect or adequate or at present worth copying; nor even that *everybody* there approves of it. He merely asserted that an attempt is being made to find a solution of a very critical problem, and that there has been progress, even though the final destination is still far off.

Current Trends in the Training of College Teachers*

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THE realization has been growing in recent years that something is lacking in the preparation of college teachers. The development of this idea is no doubt traceable in part, at least, to the rapid growth which has been experienced in college enrollments with its accompanying increase in faculty size.

The lack of attention paid in the past to the problems associated with college teaching stemmed, it seems to me, not from the feeling that all one needs for satisfactory teaching at this level is an adequate command of the subject matter in his field but rather from the conviction that the qualities possessed by the good college teacher are ones with which an individual is, or is not, endowed at birth or, in any case, are of a kind which an individual develops without outside assistance. This latter idea has, I feel, a good deal to recommend it; from which it would follow that the development of a good college faculty is to a large extent a matter of selection. Nevertheless, it seems fairly evident that given individuals with an adequate mastery of a subject-matter field and the appropriate basic personal characteristics, the conscious attention to the problems of college teaching cannot help but produce better college faculty members.

Another reason for the paucity of efforts toward improving the preparation of college teachers may be the lack of agreement on whether the graduate school or the employing institution has the primary responsibility for supplying the necessary training. The view that the graduate school should shoulder a large measure of this responsibility seems now to be quite widely held, but there is still considerable support for the opposite view that the graduate school need have no concern in this matter.

From the standpoint of efficiency of operation the graduate school would appear to be the proper place for the concentration of this responsibility. Nearly all college teachers enter the profession after a shorter or longer exposure to graduate work and to an increasing extent after completing the Ph.D. program. Looking in the other direction, college teaching represents the major employment outlet for Ph.D.'s. A study made by Ernest V. Hollis[1] showed that about 65 percent of the 20,783 individuals who received Ph.D.'s during the 1930's and were employed at the time of the survey in 1940, were in college and university work. For the field of physics the figure was about 66 percent. When one considers, further, that physics is taught at the college level in about a thousand institutions while the Ph.D. degree is awarded by about seventy-five, the conclusion seems inescapable, on the basis of economy of effort alone, that the graduate school is the place for training in the basic skills of college teaching.

The increasing concern for the quality of the preparation for teaching received by those entering the faculties of our colleges and universities is shown by the pronouncements of a number of groups which have made studies of American higher education in recent years. The President's Commission on Higher Education, while giving a good deal of attention to the quantitative aspects of the very ambitious program for higher education which it envisioned, also concerned itself with the quality of faculty personnel. The Commission, in a much quoted statement, reported: "The most conspicuous weakness of the current graduate program is the failure to provide potential faculty members with the basic skills and the art necessary to impart knowledge to others. College teaching is the only major learned profession for which there does not exist a well-defined program of preparation directed toward developing the skills which it is essential for the practitioner to possess. The objectives which higher education seeks to achieve cannot be reached unless there is realism in the programs for preparing college teachers."

Even the Association of American Universi-

* Based upon a paper given as part of a panel discussion on Methods of Training College Teachers of Physics at the annual meeting of the American Association of Physics Teachers, February 3, 1950.

[1] Ernest V. Hollis, *Toward improving Ph.D. programs* (American Council on Education, 1945), pp. 74–75.

ties, whose interests have been largely centered in scholarship and research at the graduate level, has taken cognizance of the problem and, at its annual meeting in October of 1948, recommended that "steps be taken to improve the teaching ability of those who receive the Ph.D. degree and plan to make their careers in the academic field."

Within the last few months a more concentrated attack on the problem has been made by the Conference on the Preparation of College Teachers held in Chicago, December 8–10, 1949, under the joint sponsorship of the American Council on Education and the United States Office of Education. The conference was attended by about 175 individuals who were, in the main, deans and college presidents. Six working groups formed at the conference identified and discussed the problems involved in the preparation of good college teachers, and made recommendations concerning the solution of these problems. At a final session the conference, as a whole, adopted a recommendation to the effect that there be appointed a National Commission on the Preparation of College Teachers and that this commission operate under the American Council on Education in making a large-scale attack on the problems to be solved in effecting an improvement in the preparation of college teachers.

Certain of the recommendations of the conference will be of interest to departments of physics. It was suggested, for example, that the identification and encouragement of able candidates for college teaching positions should begin at the junior or senior level of the undergraduate college and that to encourage properly qualified students to look toward a teaching career a generous fellowship program would be needed. In this connection it was felt that a program of fellowships awarded for promise of superior teaching ability would not succeed unless the stipend were substantially equivalent to those now granted to students showing promise of superior ability in research.

While there was general agreement of the need for a college teacher to know thoroughly the subject matter of his teaching field and while there was no attempt to minimize the importance of training and experience in research, a plea was made for college teachers with broad educational backgrounds. The feeling was expressed that the general education of the prospective college teacher should continue beyond the first two years of college and even into the years of graduate study. In this connection the opinion was held that general education courses of an advanced nature might be specifically designed for graduate students and that these should have no formal prerequisites other than graduate standing.

It was generally agreed also that a competent teacher must possess, in addition to competence in his teaching field, and personal qualifications which make him suitable for teaching, additional knowledge, understandings, and skills bearing upon his professional activities as a teacher. The group endorsed the proposal that professional training for college teaching "be offered as a strongly recommended elective rather than as a required course or series of courses."

One further recommendation of considerable interest is that concerning the establishment of an apprenticeship program as part of graduate training for college teaching. Apprenticeship training was understood to mean the provision of a wide variety of instructional experiences, including actual classroom teaching and student conferences, and opportunities to participate actively in course planning, testing, and a variety of relationships involving students, faculty, and the community and, in addition, some orientation to institutional structure, procedure and operation. The group felt that the apprenticeship should be an integral part of the graduate program and should be spread over a major part of the period of graduate study. It was agreed that all institutions training prospective college teachers should provide this type of apprenticeship as part of the prospective college teacher's formal program in graduate school. Conference members were in agreement, also, that the graduate school, through the departments, should be responsible for providing this experience. Though there is need of close collaboration with the undergraduate college of the institution, it was generally thought that the college should not be the responsible agency, simply because there is great danger of conceiving of the experience as getting a college job done, of narrowing the experience to a very small area of college-teacher

functions, and because, conceivably, some apprenticeship experience might be arranged for in colleges other than the one of the institution giving the graduate program.

Independently of the commissions, conferences and associations of universities which have considered the problems involved in the preparation of better college teachers, a few departments of physics have undertaken programs designed to improve the preparation of their own students for college teaching careers. The most extensive and best planned of these is the Penn State program which is described in an article by Harold K. Schilling, chairman of the department.[2]

The California Institute of Technology has instituted a Seminar on Educational Problems and Methods which is now in its third year. All members of the teaching staff and graduate students are encouraged to attend and take part in the discussions of the seminar which meets once a month during the school year. All proceedings are kept as informal as possible in order to stimulate a free give-and-take of ideas among the participants. The seminar, which is preceded by tea, is held in the sumptuous lounge of the Athenaeum where there are comfortable chairs and where smoking is permitted.

Attendance at the seminar has been good and the discussion lively and frank. While engaged on a research project at the California Institute of Technology during the academic year 1947–48, I attended the sessions of the seminar in its first year of operation and can testify as to the interest in educational matters exhibited by both graduate students and faculty. The participation of the Institute's topflight physicists in these discussions of educational problems and policies cannot help but shape for the good the attitudes of the graduate student of physics toward college teaching and its problems.

The wide range of problems considered in the Institute's seminar is seen from the topics discussed so far, some of which are: The educational aims and objectives of the Institute; How can the effectiveness of the teaching at the Institute be improved?; What steps should be taken to indoctrinate and train graduate students for teaching positions?; The advisability of introducing more electives into the (engineering and science) curriculum; The grading system—how can it be improved?; The orientation and vocational guidance of students.

Dean E. C. Watson said recently in commenting on the seminar, "So far no revolutionary changes have been introduced as a result of these discussions nor have any very important educational experiments been initiated. What has been accomplished is, instead, a better understanding on the part of the faculty of educational problems and procedures in general and of those of the Institute in particular. This has led to a greater interest in all sorts of educational matters and has caused many faculty members to give serious thought to how they could improve their own teaching."

Harvard University and Radcliffe College are experimenting with an extracurricular course in college teaching. The course, given during October, November, and December of 1949, consisted of thirteen evening lectures devoted to general educational problems and to specific teaching methods in three major areas of learning: the physical sciences, the humanities, and the social sciences.

Many departments of physics, particularly those with large general physics classes, have programs for improving the quality of teaching which are tied in closely with the general physics course. These programs undoubtedly make some contribution to the training of graduate students in the art of teaching but they differ from the Pennsylvania State College, California Institute of Technology and Harvard programs in that they are designed primarily to improve the instruction in a course and only secondarily to assist in the preparation of college teachers.

My own views on methods for the improvement of college teaching in physics can be expressed simply and briefly. In the first place, I feel very strongly that the nature of the Ph.D. program should not be altered in any significant way nor should there be any lowering of its standards in the supposed interest of improving the preparation of graduate students for careers in college teaching. The training and experience in research which the Ph.D. candidate receives give him a proprietary interest in the field and

[2] H. K. Schilling, *Am. J. Physics* 18, 549 (1950).

an enthusiasm for his subject which, to my way of thinking, are the prime requisites for good teaching at the college level. I do not subscribe at all to the viewpoint that there is any conflict between the type of training required for a research career in physics and that required for an academic career.

Improvement in the teaching of physics in colleges and universities will come about if there can be developed in our graduate students sympathetic attitudes toward educational problems, an interest in teaching, and the willingness to experiment with teaching methods in an effort to find those best suited to the individual and the teaching situation in which he finds himself. Development of these attitudes and interests depends in large measure on the possession of similar attitudes and interests by those with whom the graduate student comes in closest contact during his years of graduate study and whom he respects for their scientific competence. It is for this reason that I believe the responsibility for any program designed to prepare college teachers of physics must rest with the department of physics and that such a program should not be the responsibility of the graduate school administration nor of a department or school of education. The amount of subject matter of a professional type for which formal course work is appropriate is relatively small and can, it seems to me, be adequately covered in a seminar meeting a few hours a week for a year, accompanied, perhaps, by a colloquium of the type developed at Pennsylvania State College. Much more important is the development of proper interests and sympathetic attitudes and these, I feel, cannot be developed unless the department of physics takes an interest in the work, assumes responsibility for it and seeks assistance from competent psychologists and educators on the university staff when considering the more general aspects of teaching.

As a further suggestion of a more general nature, I should like to add that the interest of our graduate students of physics in the problems of teaching would be very considerably stimulated if they were, from time to time, to run across in the literature articles by outstanding physicists—perhaps even Nobel Prize winners in physics—on such subjects as, The Correlation of High School and College Physics, or The Function of the Lecture Demonstration in Secondary School Physics, or Present Tendencies in the Teaching of Elementary Physics. Sounds like a ridiculous suggestion, doesn't it? It might be ridiculous, at that, to expect Nobel Prize winners to concern themselves with such subjects if it were not for the fact that the subjects I chose as examples are the exact titles of three papers by one of America's Nobel Prize winners in physics; a man whose interest and accomplishments in the teaching of physics—apart from his contributions to physics itself—have earned for him this Association's Oersted Medal "for notable contributions to the teaching of physics." I refer, of course, to Robert A. Millikan.

In conclusion, I am not one who feels that a great calamity will befall higher education unless we mend our ways. I do believe, however, that college teaching can, in general, stand some improvement and the steps which need to be taken to bring about such improvement are relatively simple and painless.

In a lesser degree similar things may be said about sound. We distinguish between hearing a sound and hearing an echo of it. If the sun were as chromatically noisy as it is bright, and if terrestrial things were resonant only to certain of its notes, we should say that we were hearing the things, not the sun, when they gave characteristic sound-reflections.—BERTRAND RUSSELL, Human Knowledge, 1948.

Textbook Errors on Thermocouples

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The writer has examined sixteen first-course physics textbooks and found that of the three that attempt to explain the reversal of the emf in a copper-iron couple as the temperature is increased, all make serious errors. Several of the other thirteen also make erroneous statements of some importance on the subject. Probably many readers likewise harbor mistaken notions on the subject. This paper will first give a brief statement of the facts and then list the observed errors.

Figure 1 shows the directions and relative magnitudes of the Peltier emf, π, at the warm junction and the Thomson emf, e_T, in the copper of a copper-lead couple when the junction temperatures are 0°K and approximately 200°C. Lead has little or no Thomson emf and the Peltier emf at 0°K is zero for all pairs of metals.

The sloping line in Fig. 2 shows the thermoelectric power, P, of copper. This is de/dt, the rate of increase of the Seebeck emf of a lead-copper couple with increase of temperature of the warm junction.[1] Thermodynamic considerations show that the Peltier emf at temperature T is given by[2]

$$\pi = PT. \tag{1}$$

At the temperature T_2, π is therefore represented (Fig. 2) by the area of the rectangle $OBAT_2$ while at 0°K, $\pi = 0$. For any two junction temperatures, T_1 and T_2, the resultant Peltier emf is given by $\pi_r = \pi_2 - \pi_1$ and therefore equals the difference between areas $OBAT_2$ and $OEDT_1$. Furthermore, we see from the figure that $P = C + mT$ where C is the intercept of the curve on the vertical axis and m is the slope. Thus at either junction,

$$\pi = CT + mT^2. \tag{2}$$

The direction of π is upward in Fig. 2, toward the lead for temperatures below T_N and toward the copper for higher temperatures.

The over-all or Seebeck emf, e, of the couple is, of course, the net area under the thermoelectric power curve, i.e., $T_NAT_2 - COT_N$ if the hot junction is at T_2 and the cold one at 0°K. (In constructing the curve the choice of positive direction of e results in the fact that e may be considered to be in the counterclockwise direction in the areas mentioned, that is, in such a direction as to send current toward the warm end of the copper as far as area COT_N is concerned but toward the cold end to the extent indicated by area T_NAT_2.) Since π is more than e (for the case shown), e_T must equal the difference between π and e and be opposed to π. This tells us that e_T is toward the hot end of the copper and that its magnitude is represented by $OBAT_2 - (T_NAT_2 - COT_N)$ or CBA. Thus, if the curve is straight,

$$e_T = \tfrac{1}{2}mT^2. \tag{3}$$

For junction temperatures T_1 and T_2, e_T equals the difference between areas CBA and CED which is area $EBAD$ and is directed toward the hot end of the copper. The Seebeck emf $e = \pi_2 - \pi_1 - e_T$, and is represented by $OBAT_2 - OEDT_1 - EBAD$ or the hatched area T_1DAT_2. Its direction is counterclockwise so that the current flows toward the cold end of the copper and the hot end of the lead.

Let us consider a copper-iron couple. Copper is a so-called positive metal. The slope of its

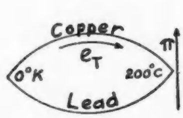

Fig. 1. Thermal emf's in a copper-lead couple.

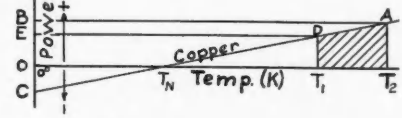

Fig. 2. Emf's in a copper-lead couple.

[1] There are gross irregularities in some thermoelectric power curves. However, provided no irregularities occur between the junction temperatures considered, they may be ignored as will be done in this article.

[2] Starling, *Electricity and magnetism* (Longmans, 1927), p. 212.

thermoelectric power curve is positive and e_T is toward its warm end. Iron, however, is a negative metal, the slope of its curve is negative and e_T is toward its cold end. (Note that e_T is always uphill in the diagram.) We see that π_2 is the difference between areas $OB'A'T_2$ and $OBAT_2$ in Fig. 3, or $BB'A'A$; and similarly that π_1 equals $EE'D'D$. The maximum value of π for a junction below T_N occurs when the temperature is $\frac{1}{2}T_N$, since the rectangular area representing π is then greatest. Thus, depending on the choice

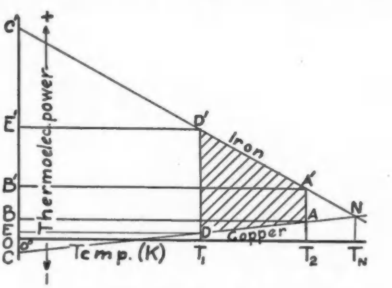

FIG. 3. Emf's in a copper-iron couple.

of temperatures, π_1 may be either greater or less than π_2.

The Thomson emf e_T in the copper is represented by area $EBAD$ and is toward the hot end while in the iron it is represented by area $E'D'A'B'$ and is directed toward the cold end. Thus the two e_T's are additive in this couple. The two π's are toward the iron. Thus the over-all emf equals $\pi_2 - \pi_1 + e_T + e_T'$, where the primed symbol refers to the iron, and is represented by the hatched area. For junction temperatures of 0°C and 200°C the various emf's are about as indicated by the lengths of the arrows in Fig. 4 and the over-all emf will be in the counterclockwise direction.

For a copper-iron couple the neutral temperature, T_N, is generally about 275°C. At this temperature for the hot junction the over-all emf e is a maximum as is shown by the fact that the hatched area will then be greatest. The Peltier emf π_2 is then zero since the corresponding area is zero. If the hot junction temperature is then increased, as in Fig. 5, π_2 reverses and e will be the difference between the hatched areas. When T_2 is as much above T_N as T_1 is below, then e will be zero (provided the lines are absolutely straight between these temperatures); while if T_2 is made still higher e will reverse. As to the direction of e, since it is counterclockwise in the hatched areas it will be such as to cause current to flow from the iron to the copper at the hot junction if the area to the right of T_N is the greater but from the copper to the iron if the area to the left is the greater.

The most common error in the textbooks examined was the idea that π at a copper-iron junction always becomes greater as the temperature increases. In fact, the three that attempted to explain the reversal of the emf agreed on this. As previously explained, when both junctions are below the neutral temperature, π may be either higher or lower at the high temperature (for ordinary temperatures it is lower), while when one temperature is above and the other below N the π's are in opposite directions.

Another fallacy is that the reversal is due to the Thomson emf increasing faster than the Peltier emf. This would imply that after the reversal the direction of the current in the couple would be dictated by the Thomson emf. Actually, the current finally flows against it. This fallacy is closely related to another that e_T varies as the square of the temperature while π varies directly with the temperature. While the former statement may be considered correct—see Eq. (3)—the latter certainly is not, as shown by Eq. (2).

One textbook describes an experiment in which one end of a copper rod is maintained at 0°C and the other end at 100°C. The author states, " . . . there will be a point P near the center at which the temperature is 50°C. If a current is then sent through the conductor (from the hot end to the cold), the point of 50° is shifted . . . towards the hot end." Since e_T throughout the rod is toward the hot end, the given current is against the emf and Thomson heat will be produced throughout the rod (because of the energy furnished by the source of the current). Thus more heat will be conducted through the colder half of the rod, setting up a steeper temperature

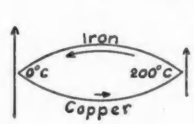

FIG. 4. Relative emf's in iron-copper couple.

gradient there. Therefore the point whose temperature is 50° will be shifted toward the *cold* end, not the hot.

An additional minor error noted was that of showing the wrong direction of current flow in an iron-copper couple at ordinary temperatures. Another book shows the Peltier emf's in a direction from the iron to the copper. One author states, "A true emf tends to cause a current to flow from low to high potential in the region in which it is developed." We will not quarrel with this statement, but the author continues, "But the pd of the Thomson emf does not act in this way," a statement which, though a little vague, certainly seems to be in error.

The final criticism which the writer wishes to make is of the practice followed by some authors of writing of Thomson potentials and pd's instead of emf's. The objection can be illustrated vividly by the familiar example of a uniform ring in which an emf is induced by dropping a bar magnet through its center. In spite of the emf no pd's would exist between points of the ring. Were the ring not of uniform construction, differences of potential would appear. Somewhat similarly, pd's do appear in a couple but the pd between the two ends of a member is not likely to be the same as the emf in that member.

The practice of speaking of thermal pd's was perhaps the cause of two of the authors making the mistaken statement that heat is produced in the copper member of a copper-iron couple in which current is flowing through the copper from the cold end to the hot because it is flowing "up a potential hill." Unless the copper member had considerable resistance in comparison with the iron the emf in the copper, being toward the hot

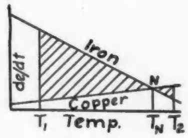

FIG. 5. Over-all emf with one junction above neutral temperature.

end, would in fact produce the potential hill referred to, but heat would be *absorbed*, not produced, in the copper, since the copper itself would be furnishing energy to make the current flow.

The *American Journal of Physics* once published[3] an article bringing out that mistaken statements in physics are often apparently copied from one text to another. Probably this partly explains the inaccuracies and poor logic so prevalent in the discussion of thermoelectric phenomena in first-year textbooks. In any case, much perplexity is caused the better students and the instructors using these texts, because it is, of course, impossible to reconcile the reversal of the emf with the stated effects of junction temperatures on the Peltier and Thomson emf's. It is hoped that this article will be of help to such readers.

[3] L. B. Tuckerman, *Am. J. Physics* 12, 75 (1944).

It is one of the most unsatisfactory features of the enormous recent developments of science, that they are so remote from all the ordinary things of life. In a few branches of science it is possible to explain to the layman what one is doing in a few words—everyone can appreciate the work of the medical researcher who is trying to cure the common cold, or of the chemist who is studying how to keep steel from rusting. But there is very little work in science that is so simple as that; for probably the man who is going to cure our colds will have spent twenty years in finding out how to keep a certain microscopic organism alive, and the chemist may be studying the effect of making the walls of a blast furnace of some different material. Neither of these occupations would seem to the ordinary man to have any special value, and the scientist often finds it hard to explain the true situation, which is that he does his work without ulterior motive, because he finds it interesting. Moreover he is a modest man, who knows that very probably nothing will come of his work, and so he does not explain that if it succeeds it will help some other work which may help some further work, which might then turn out to be of practical value.—C. G. DARWIN, The New Conceptions of Matter, 1931.

The Main Philosophical Considerations of Space and Time

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I

UNTIL recently, space and time, or space-time, were not thought of as having real existence. It is well known that, from Aristotle through Augustine, both space and time were considered only metaphysically. True, Occam and Duns Scotus advanced the notions considerably by regarding both space and time more realistically, as being part of the constituent stuff of all nature. As will be seen in the body of the paper, the naturalistic mechanical materialists (Galilei, Gassendi, D'Alembert, Lagrange, Descartes) also regarded matter and space and time as being inherently related. In the atomistic materialism of Hobbes (even in that of Joseph Priestly), and in the empiricism of Bacon and Hume, space and time were taken for granted. It was in the monumental architectonic of the Kantian critical philosophy that space and time were again thoroughly re-examined, but only to be relegated to the subjective, external, and internal senses as modes of knowing objects and successions of instants in the world of nature. Kant thought of himself as performing the Kantian revolution as an advance over the Copernican and Newtonian revolutions. In this, he could have been successful if he had followed the bent of the precritical Kant and established the groundwork for a non-Euclidean geometry. But he could not bring himself to perform this task and most post-Kantians, neo-Kantians and Kantians fared little better. The philosophers and scientists Mach, Avenarius, Cohen, Natorp, Pearson, and Poincaré, ended by being positivists of some brand or other. Their counterparts today are Schlick, Neumann, Carnap, Russell, and in some respects, Whitehead. Russell's views as to the reality of material space may not be typical of all logical positivists, but they indicate an interesting philosophical trend. "Mass is only a form of energy, and there is no reason why matter should not be dissolved into other forms of energy. It is energy, not matter that is fundamental in physics."[1]

[1] Bertrand Russell, *Human knowledge: Its scopes and limits* (Simon and Schuster, 1948), p. 291.

Perhaps it is of no real concern to laymen as to which is more important, energy or matter. The material reality of space-time, and the laws of energy and mass are, however, very real and important philosophical problems to physicists and philosophers. This importance, we know, was pointed out in 1905 by Einstein and later developed into the two phases of a single principle: that energy may sometimes be converted into matter and matter into energy. Relativity theory made known the exact amount of energy released from an atom when its mass was altered, and the exact increase in mass when it was made to move at very high velocity. Relativity, it is true, does not give a complete explanation of what happens in an atom, but a new generalized field theory, unifying gravitational and electromagnetic phenomena may take care of this. If Einstein does achieve this in his new theory, and if he is once more experimentally verified, then, once more, the material actuality of space-time will have been philosophically justified. The acceptance of such a point of view will initiate a unification so widespread that there will no longer be different kinds of physics about different kinds of space and time.

Thus, it has always been true that the problems raised by thinking about space and time have been forced upon the attentions of scientists and philosophers. Before the time of the ancient geometers, there was no concern with theoretical problems of space and time. When it became necessary to attempt some exercise of conquest over time and distance, men then began to pay attention to time as something more than the passage of days and nights. No consciously theoretical notions about space were supported until men learned that such theories had values for practical purposes. Herodotus' account of the origins of geometry arising from the necessity of redividing the land in Egypt after the annual inundations is a classical illustration of this relation of practice to theory.

II

The Oriental science which antedated pre-Socratic philosophy by a number of centuries was nonetheless concerned with and aware of the concepts of time and distance and with the properties of space, even if only as a by-product of an understandable confusion between theology and science; but it also reflected the various methods employed by the clerks and priests to make the land more prosperous. The many limitations of the Egyptian and Mesopotamian science of the Bronze Age were due to the social environment out of which the sciences sprang. Egyptian science was as interested in getting to the root of things as it was in amassing the knowledge which the caste society could use to control events in the external world. The only type of scientific attitude which could prevail was one in which control and prediction predominated. To ensure this control, the oriental scientists employed mathematical tools of a highly theoretical nature, though they may have been unaware of any theoretical implications.

We must wait until Iron-Age Ionian speculation before any attention is paid to abstract empty space and properties of numbers as such. The Ionian theoreticians were independent thinkers, free from any shackles of special circumstances, and thus they were able to rely absolutely on the facts of human experience that were available to transform general observations into the mature philosophical systems which culminated in the atomic theory of Leucippus-Democritus. Being immersed in the Egyptian and Babylonian geometry and arithmetic, they built imposing geometrical systems to fit the new and exacting urban conditions. This scientifically immutable geometry applied to the accurate right angles and triangles which they constructed. On this basis of observational induction, geometry and astronomy became sciences in the best sense, issuing forth in the pure geometry of Euclid and the theoretical astronomy of Archimedes.

The philosophical materialistic theory of space began with the physical theories of Democritus, who thought of matter as being the only real principle of the world-matter in the form of indivisibly small particles, without any quality. He called these particles *atoms*, which, he said, moved in absolute empty space. "Atoms and the Void"—this was his desideratum for the constitution of the universe. External nature was resolved into discrete, tiny, integral, dense particles, which made up all of existence. The atoms were indestructible, infinite in quantity, of all kinds of sizes and shapes. Since atoms were material and the Void nonmaterial space, opposed to one another, it followed simply that matter consisted of that which is and the Void consisted of nonbeing.

The Void, however, was necessary for the Democritean system, not as a *deus ex machina*, but as something necessary for the motion of the atoms. "Motion exists because of the Void." In this system, space was real and a necessary condition for the motion of material bodies. This notion of infinitely limitless space and its properties as being thoroughly material was the culmination of Ionian monism and the apogee of human achievement as far as scientific deduction at this stage of human development was concerned.

The remarkable feature of this metaphysical materialism was the maturity of conception in the position alloted to time. Heretofore, scientists had no conception of time as being either theoretical or as a part of nature. Yet Democritus held that time was as real as space, having neither beginning nor end, but existing in and of itself, eternally. Though Democritus considered time as a part of nature, he did not conceive of it as having any relations with space. It was objective but not material, and metaphysical but not measurable. Time was detached from matter as a self-existent reality, but it was objectively real, and thus it acquired a position it had not had before. Small wonder, then, that Aristotle with his antipathy toward Ionian science, and with his own abstract logical relation of before-and-after, should have told us only as much as he thought was necessary concerning the Democritean theory of time.

The exponent of atomism after Epicurus and Lucretius was Gassendi, who also declared that space was objectively existent, being disunited with matter and without motion and substance. Gassendi's niche is secure in the history of atomism and materialism, but it must be said that he did not make any significant advance in

his conceptions of space and matter in their relationships with one another.

It was Gassendi's contemporary, Descartes, who developed a dualistic philosophy which was idealistic when he considered the origin of knowledge and materialistic when he discussed the reality of physics. Descartes confirmed the universal connection of all phenomena in nature; explained the development of the material world purely mechanically and in a way quite different from that of the Greek atomists. This mechanical materialism regarded space as corporeal, material substance because it was inseparably connected with material bodies and their extension in space. "Space or internal place and the corporeal substance which is contained in it, are not different otherwise than in the mode in which they are conceived by us. For, in truth, the same extension in length, breadth, and depth, which constitutes space, constitutes body."[2]

All bodies, whether particular or universal, were phenomena which were reducible to physical and mathematical terms. However, like Hume at a later time, Descartes felt compelled to deny empty space, since such a conception involved an illusion or an apparentness of space. "As regards a vacuum in the philosophic sense of the word, i.e., a space in which there is no substance, it is evident that such cannot exist because the extension of space or internal place is not different from that of body."[3] Moreover, there was no material space which was not in motion and no motion without material space. Cartesian space was the property of material bodies in extension, therefore material space was all that there was.

Descartes' view that space was illimitable as far as divisibility was concerned was at variance with earlier notions, but it was consistent with his notion of the divisibility of matter in depth. In upholding the identity of space and matter, rather than assuming their unity, Descartes advanced metaphysics, but he retarded atomic theory. He believed that it was possible to know "the force and action of fire, water, air, the stars, the heavens and all the other bodies that surround us,"[4] and in so doing he advanced the scientific method by attempting to set free the scientific intellect to master the world of nature.

As might be expected, Descartes related time to the spiritual substance because he regarded it as a mode of thinking substance. "Thus time . . . which we describe as the measurement of movement, is only a mode of thinking." Not only was this dualism, it was an endorsement of an idealist concept of time precisely because it was separated metaphysically from space and from matter. Perhaps it was too much to expect that Descartes should have made any greater advance in this respect than his contemporary, Newton.

Newton's and Galilei's ideas of space and time were put forward in opposition to Cartesian physics, as a continuation of the complete overthrow of Aristotelian science. Newtonian physics, along with Galilean mechanics, supplied the basis for the foundation of classical physics, a new conception of nature, a particle physics in which sensed colors, sounds, and odors do not belong to the actual objects themselves, but are only appearances secondarily apprehended by the observer. Galilei's views about sensed qualities in sensed space and time meant for him, a separation between the actual objects of space and the independent observation of these data by the observer. To use Galilei's terminology they were factors such that if the observer was removed, nothing of them would remain.

> "Nevertheless I say, that . . . I feel myself impelled by the necessity, as soon as I conceive a piece of matter or corporeal substance, of conceiving that in its own nature it is bounded and figured in such and such a figure, that in relation to others it is large or small, that it is in this or that place, in this or that time, that it is in motion or remains at rest . . . in short by no imagination can a body be separated from such conditions; but that it must be white or red, bitter or sweet. . . . I do not perceive my mind forced to acknowledge it necessarily accompanied by such conditions; so if the senses were not the escorts, perhaps the reason or the imagination by itself would never have arrived at them. Hence I think that these tastes, colours, etc., on the side of the object in which they seem to exist, are nothing else than mere names. . . ."[5]

This separation of the qualities of the object and the object itself was carried further by Lockean sensationalism which conceived of infinite space and time as negative ideas. Galilei's work was

[2] *The philosophical works*, Translated by Elizabeth S. Haldane and G. R. T. Ross (Cambridge, 1931), vol. 1, p. 259.

[3] See reference 2, p. 262.

[4] Rene Descartes, *Discourse on method*, pt. VI.

[5] Galileo Galilei, quoted in Burtt's *Metaphysical foundations of physics* (Harcourt, Brace, New York, 1925), p. 75.

accomplished in order to triumph over Cartesian dualism, and in the course of it Galilei presented the world with an additional term in our knowledge relation. Seventeenth century physics represented that stage in social nature where a Hobbes could carry this through to the logical conclusion of regarding human beings themselves as atomized social particles.

Newton followed Galilei closely in his ideas of space and time. He believed that they existed objectively, but their existence was in no way dependent on the believer's conception of them. However, he retreated from this materialist position when he viewed matter as independent of space and time, which are separated from it and exist independently, isolated from matter and each other. Moreover, the motion of matter, he thought, proceeded independently of space and time. In the beginning of the *Principia*, Newton wrote:

"I do not define time, space, place and motion as being well known to all. Only I must observe that the common people conceive these qualities under no other notions but from the relation they bear to sensible objects. And thence arise certain prejudices for the renewing of which it will be convenient to distinguish them into absolute and relative, true and apparent, mathematical and common."[6]

And further:

"I. Absolute, true and mathematical time, of itself and from its own nature flows equally without relation to anything external, and by another name is called duration. . . .

II. Absolute space, in its own nature without relation to anything external remains always similar and immovable."

As conceived by Newton, the cosmos was "a vast mathematical system whose regular motions according to mechanical principles constituted the world of nature." In relation to this world-view, man was only an automaton, a receptacle for all those sensed qualities which could not be reconciled with any common sense view of the world of phenomena.

Newton considered gravitation to be remote action through empty space. He did not say what the cause of the gravitational force was, because he could not experimentally have verified his assertion by means of observations. Although in his second law of motion he assumed the existence of "a very fine ether" permeating every material body, he specified that the sum total of the effect of one physical object upon another physical object was to change the velocities of the interacting objects. Therefore, his point of view did not imply that space was filled with matter.

Newton's conception of space and time was metaphysical-materialist, both because of his assumption of the existence of active forces independent of matter which he thought of as inert and because of his assumption of a three-termed relation of the appearance of space and time. He was led to the further assumption of some divine primary impulse without which the universe or the solar system could not have originated. Under this unchanging impulse the planets preserved their initial relative distances and distribution. Space became "the sensorium of God," and Newtonian mechanics led straight to religion and idealism.

Kant raised the philosophical problems of space and time to their highest metaphysical-idealist expression. Kant did not state the problem of knowledge in spiritual terms, but he did state the problems of space-time in subjectivist terms, even though the transcendental aesthetic was mainly about the impossibility of knowing space and time. Kant declared that neither space nor time could be classed with the data of the bodily senses or with the concepts of the understanding. They were sensuous, not abstract but concrete, not modes of thinking but modes of existence; yet at the same time, they were *a priori*.

Space and time, then, exist in and by themselves. Each was unique in itself, each was an infinite existence. When they were looked at as things in themselves, they were, in Kant's own phrase, monstrosities (*Undinge*). The transcendental aesthetic, "the science of all the principles of sensibility *a priori*," described space-time relations, "the two pure forms of sensuous intuition as principles of knowledge *a priori*."

Time, like space, was a representation that was purely subjective, like an internal sense and intuition. "Time is not an empirical conception, which is presupposed in all perceptions." "Time

[6] Sir Isaac Newton, ***Mathematical principles of natural philosophy, and his system of the world.*** Revised translation by Florian Cajori (University of California Press, Berkeley, 1934).

is nothing else than the form of the internal sense, that is, of the intuitions of self and of our internal state." "Time is not a discursive or general conception, but a pure form of sensible perception."

In Kant's precritical period, he had tried to deduce the three-dimensional character of space as a consequence of Newtonian gravitational laws. After his adoption of the critical view of space-time, he seemed to have rejected any such possibility. All space and any space must be Euclidean; the uniformity of space was a presupposition of the *a priori* certainty of geometry as an intuitive science but not as a science arising out of any empirical observations. Kant's avowal that he did not reject the empirical reality of space and time, but that he denied only that they could be given to us as the objects of our senses, must be taken seriously.

Kant's conception of space and time was not only subjectivist to an extreme; his attempted reconciliations with other spaces and times disclosed to him the impossibility of regarding space and time in any other light than that of pure intuition, *a priori* forms of representation of the subject. Moreover, any observation that Kantian space and time nevertheless remain infinite in character must follow from Kant's statement that our original conceptions of them are postulated as infinitely unlimited. Such an attribute, of infinity is antecedently given as existing *a priori*, which is conjoined with the entire Kantian *a priori* conception of space and time.

The post-Kantian philosophers were just as much concerned as Kant with the problems generated by regarding space and time as modes of thought or as reflections of objective reality. Both Mach and Avenarius, wishing to disguise their materialism, called their philosophy of knowledge and matter empirio-criticism. Mach, for example, regarded consciousness and sensations as the ultimate elements of the world and not as phenomena of the external world. He regarded space and time as subjective consciousness, conditioned by the sensations. "Time and space are interdependencies of physical elements."

Mach went much further than the precritical Kant in trying to establish the existence of physical and chemical elements, outside of actual, three-dimensional space. He believed that we "need not necessarily represent to ourselves molecular processes spatially, at least not of three dimensions."[7] And, "when, however, we operate with purely abstract things, such as atoms and molecules, which by nature cannot be disclosed to our senses, we have no right necessarily to think of these things in relationships, i.e., in relative positions which conform to Euclidean three-dimensional space.[8] It would seem that this sensationalism and mechanical materialism sought for its refuge all of the arguments against the establishment of the view that space was relative and real at every moment to a world-observer. Mach attempted to show that space could not be real in a world of atoms and elements and that physical space must therefore be abandoned. This was an idealistic operation performed by mechanical materialism upon physics by destroying any vestige of independent reality which had been preserved by Kant's noumena. Sensations were enthroned as the ultimate orderable elements of the physical world, and science became a logical construct system based upon the "elements" of sensations.

These attempts to deny the primacy of matter and the objective nature of mathematical science were opposed by Marx and Engels and their followers, dialectical materialists who believed that the universe itself was matter in motion and that matter and motion could not be separated from each other. Since, in this conception, moving matter existed in space and time, it was declared that space and time were the objectively real forms of existence. Such a conception, Marx and Engels declared, was founded on the observations of physical science which have been attested to throughout the historical development of science and human knowledge. Science and human knowledge have established as empirical facts this objective existence, which have been concurred in by the achievements of physics, geometry, and mathematics. Thus, the existence of space and time have been established by these physical sciences, but this existence was not dependent on man's

[7] Ernst Mach, *History and root of the principle of conservation of energy* (Open Court, 1911), p. 86.

[8] *Erkenntniss und Irrtum, Skizzen zur Psychologie der Forschung* (J. A. Barth, Leipzig, 1920), p. 418.

thought of them or on his methods of measuring them. Their properties depended solely on objectively existing matter. Space and time were interrelated to the extent that they manifested a unity immediately in the material world and could neither be separated or sundered.

These generalizations were more upsetting than controvertible since they were the outgrowth of a strict materialist outlook. Engels put it very simply when he declared, "The materialist outlook on nature means no more than simply conceiving nature as it exists without any foreign admixture." Mathematics and physics were the sciences which were concerned with the real world, mathematics because it deals with the quantitative relations and spatial forms of the objective world and physics because it deals with the physical aspects of the inertness and movement of the material world. In the same way, geometry as the analysis of spatial forms assays the general properties of matter itself, when it is concerned with the physical nature of space.

III

Before there could be any new contribution to the study of the physical nature of space, there had to come into being revised or new geometries and such geometries were not long in making their appearance. Kant, as noted above, in his precritical period, had been on the verge of new geometrical interpretations. These new geometries, once they were put forth as respectable theories about spatial forms, developed into every possible shade of mathematical reasoning from the audacious concepts of Gauss to qualitative geometry, the science of topology. These physical generalizations and discoveries had for their goal a singlemindedness about space-time, stating that they were unalterably connected and that they were the basis for any real, deep knowledge of the internal nature of the universe. Instead of the Kantian view that mathematics and geometry were known only through intuition, the new views suggested now that there could be other spaces than Euclidean. After some geometers had proved that our space was Euclidean in character, others came forward to prove, upon analytic and empirical investigations, that our space was also non-Euclidean in character. Any non-Euclidean geometry in which postulate V had no place constituted a valid, logically consistent system.

These new geometries raised serious and important philosophical questions. The Euclidean geometry had become established as the Platonist philosophy of logical and mathematical harmony, with its emphasis on unswerving and invariant relations. Kantian idealism had also established the idealist viewpoint that the conceptions of geometry were intuitive and *a priori* in nature. The coming into being of the non-Euclidean geometries meant that geometrical axioms have value and retain their validity only in the event that they mirror definite, actual properties of real space and spatial configurations. For example, the Lobachevski-Bolyai geometry was realized in a group of spatial figures of negative curvature, applied on a surface, the Beltrami "pseudo-sphere," while Riemann's geometry was applied in spherical spaces.

The nineteenth century ushered in another revolution in geometry with the new science of topology (*analysis situs*)—a science that studied the properties of space and of spatial figures which remain unchanged under all continuous transformations. This kind of analysis indicated that even *more* could be discovered mathematically concerning the properties of real space, thus adding to the knowledge of spatial objects by the study of the nonmetrical property of dimensionality. One-dimensional objects (linear figures), two-dimensional objects, nondimensional objects and such empirical objects as psychological data were studied from the point of view of those properties that were dependent on a corresponding number of dimensions. The property of the connectedness of spatial objects, a property which reflects the continuity of spatial objects, was also examined in topology.

Riemann's topological investigations went beyond the theorizing of Euler when he pointed out the value of topology for the theory of functions. In the hands of physicists such as Milne and Dirac, the theory of functions gave even greater value to such considerations of space-time connectivity of surfaces and time irreversibility.

Topology, then, manifested the remarkable properties requisite for a basic science of space,

since positional relationships are not fundamentally measurable. Moreover, a metricized geometry stated in terms of a topological equality of circles and ellipses displayed every possible type of demonstrable proof for the spatial relationships of any closed three-dimensional figure.

IV

The theory of material fields became one of the most fruitful notions in modern physics because it linked together space and matter. Faraday, in introducing the theory, thereby contradicted Newton's *actio in distans* theory. The general theory of relativity established the complete dependence of the properties of space on the distribution of matter in it. Wherever there are masses of matter, there is a gravitational field which is continuous and which acts at absolutely every point of space. Einstein held that it was impossible to conceive any point in space where there was not a gravitational field. Points in space may be conceived of where there is no electromagnetic field and the pressure of neutrons inside the atom suggested the existence of places in space without an electromagnetic field. Einstein did "regard an electromagnetic or gravitational field as a physical reality, in the same sense that matter had formerly been considered so. The theory of relativity teaches us the connection between different descriptions of one and the same reality."[9] Modern physics, with its theory of material fields, reflecting the property of the continuity of matter in space, bears out the contention of the inseparable connection of space and matter.

The basic concepts of the relativity theory confirmed the materialistic interpretation of space and this theory revised radically all of the former views of classical physics on space and time. Einstein regards the universe as a single unit in which space and time are inseparably connected. The cofounder of the theory of relativity expressed this as follows: "Henceforth, space by itself, and time by itself, are doomed to fade away into mere shadows and only a kind of union of the two will preserve an independent reality. The older independent space-interval (distance) and the independent time-interval between any two events, is replaced by the conception of the space-time interval."[10] Einstein referred to the absolute character of the space-time continuum in contrast to absolute time and absolute space by considering motion only in relation to a continuous material world. "If the principle of relativity were not valid we should therefore expect that at the direction of motion of the earth at any moment would enter into the laws of nature and also that physical systems in their behavior would be dependent on the orientation in space with respect to the earth. . . . However, the most careful observations have never revealed such anisotropic properties in terrestrial physical space, i.e., a physical non-equivalence of different directions. This is a very powerful argument in favor of the principle of relativity."[11]

The motion of the so-called world-point also occurs in this space-time world. The change of its position in the universe registers at the same time in-space-coordinates and in-time. Einstein used the term space-time event to mean the time of the event as well as the three coordinates in ordinary space; thus "events" are occurrences like any light signal in the world. The Lorentz transformations showed that the equations of this world-point led to the conclusion that events shown as simultaneous in one system are not so in the other, and vice versa. The simultaneity of events and the constancy of distance are relative.

The universality of the relative character of the motion of bodies was guaranteed since there was no motion of a body unrelated to others and unreferred to others. The motion of a body was possible only in relation to some definite medium. In this relative motion, the measurements of space and time were dependent on the corresponding motion of the bodies of systems. Relative measurement of time and space intervals only complement the objectivity of space and time. Moreover, the relative character of simultaneity and space-intervals depended on the velocities with which different systems travelled with respect to one another and not on the perception of an observer or on his subjective

[9] Philipp Frank, *Einstein: His life and times* (Knopf, New York, 1947), p. 216.

[10] Hermann Minkowski, "Space and Time," in the *Principle of relativity:* A collection of essays by Lorentz, Einstein, Minkowski, and Weyl, (Dodd, Mead, New York, 1923), p. 28.

[11] Albert Einstein, *Relativity* (New York, 1920), p. 15–17.

qualities. Space and time were no longer regarded as absolute, or as independent forms of the being of matter; they were now absolute in their mutual affinity, in their inseparable character, as a unitary space-time continuum. As Einstein repeatedly declared, the absoluteness and objectivity of space and time under the special theory of relativity now appear under a different guise no longer in the old metaphysical way, but in a dialectical connection.

Since the special theory of relativity required a new geometry to replace the old Euclidean geometry, this has often led scientists to assert the dependence of the physical on the geometrical and thus to conclusions concerning the finiteness of space. Einstein is explicit on this point: "*But* speculations on the strict use of the universe also move in quite another direction. The development of non-Euclidean geometry led to the recognition of the fact that we can cast doubt on the *infiniteness* of our space without coming into conflict with the laws of thought or with experience of Riemann and Helmholtz." If Einstein suggests that the laws of finite space are applicable only within the sphere of action of given centers of masses of matter, it seems unlikely that the theory of relativity applies also to any deductions concerning infinite space. However, there may be some doubt as to the possibility of deriving the properties of space from Riemannian geometric properties alone. It would seem that, since the only substance of all changes is matter, that the properties of space are determined only by matter itself.

There are those physicists and philosophers who imagine the possibility of a motion out of space or out of time as real events. Sir James Jeans, for example, believes that we can never know the real world, and theorizes that there is a "substratum lying beyond the phenomena, and so also beyond our access, in which the happenings of the real world are somehow determined."[12] In Jeans' view, matter is not objective nor is it absolute, nor is it the sole substance and the cause of all changes. According to Jeans, "There is a certain presumption—although certainly no proof—that reality and knowledge are similar in their natures, or in other words, that reality is wholly mental."[13] Yet he can say that, "if our minds go out of existence, or cease to function, the stars, bricks, atoms, continue to exist and are still capable of producing perceptions in other minds. On this view space and time have just as real existences as these material objects; they existed before mind appeared in the world and will continue to exist after all mind has gone."[14] These ideas are in contradiction and too often reflect an attempt to found an emotional philosophic attitude in scientific theory. Strangely, such ideas have gained prestige because they are closely connected with a materialist theory of time, space, and absolute matter.

Whitehead attempted to encourage Einstein to revise his relativity theory so as to give it a different philosophical-metaphysical angle. According to relativity, motion occurs only as a relative motion of bodies and depends on the curvature of space. The objectivity of motion in the philosophical sense is not in contradiction to the relativity of motion in the physical sense, and yet we are told by Einstein's biographer that Whitehead repeatedly urged Einstein to forego the notion of space curvature. "Einstein, however, was not inclined to give up a theory, against which neither logical nor experimental reasons could be cited . . . Whitehead's metaphysic did not seem quite plausible to him."[15] Indeed, in *Process and Reality*, Whitehead regarded space perceived in "presentational immediacy" as being no longer objective space, but rather as an aspect of the percipient occasion itself. Yet, in respect to time, we are told, "For Whitehead time is the relational and logical aspect of change; it is a set of relations which is internal to fact. He repudiates with vigor the Newtonian theory of absolute time as a real flowing container of facts."[16]

The relativity of space and time, according to the materialist viewpoint, is dependent, first of all, on the relative character of motion, and second, on the mutual interdependence of space and time. The viewpoint is that of the relativity of space-intervals and the relativity of simultaneity from the standpoint of material systems

[12] Sir James Jeans, *Physics and philosophy* (Cambridge Press, Macmillan, 1943), p. 141.

[13] See reference 12, p. 203.

[14] See reference 12, p. 58.

[15] See reference 9, p. 189.

[16] Wm. W. Hammerschmidt, *Whitehead's philosophy of time* (Kings Crown, New York, 1947), p. 7.

travelling at different velocities in relation to one another. The objectivity of these intervals remains and the fact of this objectivity is seen from the point of view of differently moving systems. The general theory of relativity applies to every principle in the theory, including the special theory and affirms the objectivity of space and time.

A fundamental position of materialism with respect to space and time is the fact of their being measurable. Intervals exist, whether the methods of measuring time-intervals are external or whether they are of a presented locus in presentational immediacy. The changes which occur in intervals depend on objective factors; the many methods for measuring time in no way disprove the objective existence of time. The time of the revolution of the earth around the sun, for example, is a period which depends on the properties of the motion of material bodies and on nothing else.

Many philosophers, idealists for the most part, who postulated a finite space and time, usually ended with a recognition of a supernatural power and a transcendental realm which contradicted all of the progress of human knowledge. Einstein at one time asserted that space was finite because of his belief in the curvature of space, but later he renounced this view because of the support which it gave to idealists and theologians who used his hypothesis and equations to attack the infinity of space. Einstein saw that his continued espousal of finite space indicated a fallacy, the mechanical application of the laws of the finite to the infinite. Bergson, for example, attempted to deduce this finiteness of the universe from other principles, notably his *élan vital* and his *Duration*, as a result of which the universe arises and receives its motion.

Among those philosophers who find it necessary to postulate the infinity of space and time, who reject vitalism, entelechies and impulses, but who nevertheless feel impelled to interject some philosophical explanation about this infinity, no one is more convincing than Alexander. This same alleged rejection of a metaphysics, because it remains closest to a physical theory, has nevertheless, all the signs of a metaphysical principle. Alexander's space-time hypothesis emphasizes both change and infinity, as it is the creative force in the world, the substratum of which all existents are composed. Space and time have no reality apart from each other, but of the two it is time which is the source of motion.

"Space and time may not be merely considered apart from the bodies or events which occupy or occur in them, but they really exist apart or are realities simpler than these bodies or events. . . . The hypothesis is then, that space-time is the stuff out of which all existents are made. Existents are complexes of space-time, that is, of motion; they are, as it were, crystals within this matrix or eddies within this vast whirlpool. As time goes on, higher and higher complexes of the spatio-temporal stuff emerge with qualities, the scale of such qualities, e.g., materiality, colour, life, mind whether it begins with materiality or at a simpler stage, being itself unending."[17]

Alexander's space-time is not material, however, since like Bergson's *Becoming*, it cannot be matter if it is the source of matter. This space-time, then, is nonmaterial and is the agency of what Alexander calls the quality of "deity," though it does not account for the emergence of matter. This space-time is the permanent substratum of change; and it is pure motion in alignment with nothing else in the universe. It transcends empirical reality and is nothing if not a *nisus* which becomes a metaphysical *Weltanschauung*. This may not be antiscientific in the same sense as those who would deny completely the theory of infinite space, but such theorizing appears to be at variance with science, and above all, with the unchanging, absolute law of nature, that nature is eternal, indestructible, and uncreated. If Alexander's philosophy ends up as a disguised theology, then, despite its resemblance to scientific physical theory, as a philosophy it reflects a diametric opposition which is irreconcilable.

According to the theory of relativity, the dimensionality of a space depends upon the number of solid, three-dimensional bodies chosen. This dimensionality has a real connection with the laws of motion of material bodies, and with the laws of spatial translation (in which all the points of the body in motion possess at any instant the same velocity and direction of motion) and this indicates in no way a rejection of space as three-dimensional. Events are used to describe physical bodies in motion, and since all events are char-

[17] Samuel Alexander, "Space-Time," *Proc. of Aristotelian Soc.* **18**, 414-16 (1917–18).

acterized by four dimensions (including the time of their occurrence), the space of the theory of relativity is still the same three-dimensional space. It should be mentioned here, that though Whitehead's analysis of nature is that of a four-dimensional world, he does not include in this analysis the mechanics of the theory of relativity. Whitehead's "abstractive sets" are four-dimensional "regions," expressing spatial and temporal regions, but only as a parallelism of extension.

The connectedness of real space as a geometrical property exhibits the physical continuity of space and is thereby defined by that property. Connectedness implies contiguity when there is infinite external spatial connection. Thus, there is no real separation of contiguous spatial regions. The difficulties of points is overcome in topology by the concept of physical continuity (material fields) and with the consequent facts of real velocity. Whitehead's theory of extensive abstraction is more in line with this view and he expressed it in the following way: "In place of emphasizing space and time in their capacity of disconnecting, we shall build up an account of their complex essences as derivative from the ultimate ways in which these things, ultimate in science, are interconnected."[18]

Many men have written about time, the time-process and about the endlessness of duration, but there have not been many attempts to explain the physical nature of time. Whitehead has taken time seriously as demanding the attention it deserves, Bertrand Russell quite the contrary. Temporalists have made a fetishism of time and change which does not become science or physical theory, but theology, as, for example, Alexander who believes that time is unthinkable from the experience of time. Alexander's attempt develops into a tendency to get around the distinction between the past and our memory of the past. And thus time cannot be considered as an objective reality. It is nothing but a rejection of time and change.

For Whitehead, however, since the reality of change is undeniable, his philosophy of time as an aspect of the physical space-time continuum is always an objective consideration. It is only in his abstract conceptualizing about passage, becomingness, process, causality, extension, transitivity, presentational immediacy, that his philosophy of time takes on the characteristics of metaphysics.

The one-dimensionality of time means that it is adequately depicted by a single coordinate, analogous to the dimensionality of space. A single measurement along a single temporal coordinate adequately defines the movement of a material body in time. Thus, time irreversibility means the quality of the flow of time in a single and unique direction, and, as Milne and Dirac point out, the content and direction are irreversible. As J. B. S. Haldane says, we are able to make distinctions between past, present, and future time, which proceed in a single and definite direction and are never interchangeable. Irreversibility is the ineluctable property of time, and as Whitehead agrees, is symptomatic of the life process in general. This irreversibility of time is associated with the phenomenon of bound energy, where free energy is constantly decreasing and entropy is increasing, where there may be an approach to thermal equilibrium. Or, to put it another way, the temporal order in the universe is continually decreasing and the disorder continually decreasing which is one side of the physical basis for the irreversibility of time. There would not be the "thermal death" of the idealists-theologians, but there would be the ever-present possibilities of cyclical trends and continuous flows of energy through the universe.

The most important aspects of space and time, general and special, have been considered. There is no claim to exhaustibility, nor can there be. For the properties of space and time are inexhaustible and manifold, just as is the case with matter. New discoveries of space and time will be made, possibly electrical, sonic, chemical, biological, and even mathematical. The task of physical science is to see the unfolding of all these possibilities on the basis of a materialist outlook of the world, a *Weltanschauung* with a materialist world outlook.

Postscript

In 1905 Einstein pointed out that, in accord with the principle of relativity there was no very

[18] Alfred N. Whitehead, *Enquiry concerning principles of natural knowledge* (Cambridge University Press, London, 1925), p. 4.

real difference between mass and energy, but that each has almost equivalent amounts of the other. This proof was not based on experimental evidence, but on excellent assumptions, and strong indirect evidence.

The Conservation of Mass and Energy

"There are two principles that have been cornerstones of the structure of modern science. The first—that matter can be neither created nor destroyed, but only altered in form—emerged in the nineteenth century and has since been the plague of inventors of perpetual-motion machines; it is known as the law of conservation of energy.

"These two principles have constantly guided and disciplined the development and application of science. For all practical purposes they were unaltered and separate until some five years ago. For most practical purposes they still are so, but it is now known that they are, in fact, two phases of a single principle for we have discovered that energy may sometimes be converted into matter and matter into energy. Specifically, such a conversion is observed in the phenomenon of nuclear fission of Uranium, a process in which atomic nuclei split into fragments with the release of an enormous amount of energy. The military use of this energy has been the object of the research and production projects described in this report."[19]

[19] Quoted from the full text of the Official Report, *Atomic energy for military purposes* by Henry D. Smyth (Princeton University Press, 1946), pp. 1–2.

Physical Ideas, their Content, Logic, and Social Contexts in the Education of Humanities Majors at Wesleyan University*

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THE many scientists and teachers who have devoted much time and effort to the "general education" in science of nonscience majors have evolved several different approaches to this problem.[1] Dean McGrath, now U. S. Commissioner of Education, edited a volume which consists of reports from twenty-one colleges on their theoretical and practical solutions to this problem.[2] President Conant devoted his Terry Lectures at Yale to the general citizen's pro' :em of understanding science; and he has since collaborated with Professor Roller and others in the working out of case studies in scientific discovery, several of which have recently appeared.[3] Foundations and universities

* Parts of this paper were presented at the June, 1950 meeting of the AAPT in Middletown, Conn.

† Opportunity to develop certain of the ideas reflected in this paper as well as to spend a year in uninterrupted study was made possible by a postdoctoral fellowship in philosophy of science during 1949–1950 from the American Council of Learned Societies.

[1] Excellent bibliographies on general education, including science, are provided by E. J. McGrath, "A bibliography on general education," *Educ. Rec.* **21**, 96 (1940) and W. N. Lyons, "A further bibliography on general education," *J. Gen. Educ.* **4**, 72 (1949–50). A brief list of those papers which have been of particular stimulation to the total context of this paper follows. They are strongly recommended to those who wish a quick thorough account of the chief current views: D. Roller's early and excellent "The role of the sciences in general education," *Am. J. Physics (Am. Physics T.)* **6**, 244 (1938); S. J. French on the Colgate approach which utilizes a core of particular technical problems, "Science in general education," *J. Gen. Educ.* **1**, 200 (1946–47); E. C. Kemble's description of his Harvard course for superior students which stresses analytic as well as historical thinking, "A general education course in physical science for superior students," *J. Gen. Educ.* **2**, 322 (1947–48); J. J. Schwab of the University of Chicago on the epistemological problems of science as a basis for educational procedures, "The nature of scientific knowledge as related to liberal education," *J. Gen. Educ.* **3**, 245 (1948–49); E. M. Rogers on his Princeton course, "Sample *versus* survey in physics courses for liberal arts students," *Am. J. Physics* **14**, 384 (1946) and "Science courses in general education" in the cooperative volume *Science in general education* (see reference 2); the work of J. B. Conant's group (see reference 3); the important and little noticed discussions by J. D. Bernal, "Science teaching in general education," *Science Education* **29**, 233 (1945) and "Science and the humanities," *Universities Quarterly* (May, 1947) as reprinted in his selected essays *The freedom of necessity* (Routledge and Kegan Paul, London, 1949), pp. 135–161.

[2] E. J. McGrath, ed., *Science in general education* (Wm. C. Brown Company, Dubuque, 1948).

[3] J. B. Conant, *On understanding science: An historical approach* (Yale University Press, New Haven, 1947). This was followed by his "progress report on the use of the case method in teaching the principles of the tactics and strategy of science," *The growth of the experimental sciences: an experiment in general education* (Harvard University Press, Cambridge, 1949); and the *Harvard case histories in ex-*

have sponsored conferences for those who teach or direct such programs. And eminent philosophers (among them Dewey, Northrop, and Frank) and scientists (among them Einstein and Oppenheimer) have recently written careful reflections on the diffusion of scientific knowledge and scientific spirit.[4] Those who have followed these discussions—and physicists have been among those most concerned—will recognize the new jargon: case-studies, block-and-gap, single science, integrated studies, philosophical implications of science, core course, science and society, strategy and tactics of science, among them. That phrase which best defines the subject of all this concern is George Sarton's "the life of science."[5] It has been with his emphasis in mind that a two-semester course in physical science (complemented by one in the biological sciences) has been developed at Wesleyan University, initially by J. W. Abrams and now by me, with the aid of a small faculty committee.[6]

Goals of the Course

We have maintained several ambitious goals despite the evidence from many other colleges that the attempt at simultaneous achievement of these goals has led to extremely superficial survey work. Why we have done so will be shown here. The goals are these:

1. To give a direct acquaintance with the actual nature of the physical world via those aspects of the physical sciences which have on one count or another been considered crucial. No amount of emphasis on methods, implications, history, philosophy, or social functions should obscure the evident necessity for the educated citizen to understand certain key unavoidable empirical facts about the world he lives in. It is true that too great a number of these facts and too much theoretical and experimental evidence lead to the old and objectionable survey course. However, to eliminate knowledge of the nature of the world from the primary purposes of this course would be to caricature education in the physical sciences. Even the most "gappy" of the "block-and-gap" programs and the most intensive of the case-histories courses are supposed by their sponsors to provide this knowledge, but to do so incidentally to their particular techniques.[7] A further reason for this emphasis on the *knowledge* which science has gained lies in our elementary conviction that the significance of a subject cannot be understood without understanding the subject itself—a principle of the course which distressed some students considerably!

2. To show the logical structure and status of present-day scientific knowledge. We want to discuss the nature of scientific knowledge as well as the nature of scientific discovery, and hence we must treat the *logic* of science as distinct from the methodology of science (and *a fortiori* from the history of science). Regardless of the means of attainment of current knowledge, it is an historical phenomenon which lays claim to objective validity and it has characteristics which may be analyzed. The analysis of science, whether conducted by the logical empiricists (who summarize the goal of all legitimate phi-

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perimental science: Case 1, *Robert Boyle's experiments in pneumatics* (ed. J. B. Conant); Case 2, *The overthrow of the phlogiston theory: the chemical revolution of 1775–1789* (ed. J. B. Conant); Case 3, *The early development of the concepts of heat and temperature* (ed. D. Roller); Case 4, *The atomic-molecular theory* (ed. L. K. Nash), (Harvard University Press, Cambridge, 1950).

[4] John Dewey was always concerned with the education of modern man in scientific ways of thought. See, e.g., his *How we think* (Heath, rev. ed., Boston, 1933), *Experience and education* (Macmillan, New York, 1938) and "Challenge to liberal thought," *Fortune* **30**, 155 (1944). See also: F.S.C. Northrop's *The logic of the sciences and the humanities* (Macmillan, New York, 1947) esp. Ch. 1–7, 20, 22–23; Ph. Frank's *Modern science and its philosophy* (Harvard University Press, Cambridge, 1949) esp. Ch. 13–15; A. Einstein's *The world as I see it* (Covici, Friede, New York, 1934), *Out of my later years* (Philosophical Library, New York, 1950), and his instructive and systematic attempts to formulate scientific theories for the lay student, e.g., *Relativity* (Holt, New York, 1920 and Hartsdale House, 1947) and, with L. Infeld, *The evolution of physics: The growth of ideas from early concepts to relativity and quanta* (Simon and Schuster, New York, 1938); J. R. Oppenheimer's "Physics in the contemporary world," *Bull. Atomic Scientists* **4**, 65 (1948).

[5] G. Sarton, *The life of science: Essays in the history of civilization* (Henry Schuman, New York, 1948).

[6] Professor Abrams has described his version of the course in the cooperative volume edited by McGrath (See reference 2, pp. 265–280) and I shall confine myself to the present course in physical science.

[7] E. M. Rogers writes: "Many of us doubt if we can—or even should—teach *the* basic principles and their applications . . . the particular choice may not be very important"; and again: "show what science is like, what scientific procedure is like, and what scientists are like—these, I think, are the most real and probably the most important things we expect from a science course" (see reference 1). And J. B. Conant writes: "The course [using case methods—R.S.C.] would not aim to teach science—not even the basic principles or simplest facts—though as a by-product considerable knowledge of certain sciences would be sure to follow" (reference 3).

losophy as precisely this analysis of science), or the neo-Kantians like Cassirer and Margenau, or idealists like Eddington, or realists like Einstein, or others, provides the genuine bases for hope of the extension to all inquiry of the scientific temper. These analysts of science are concerned with the old philosophical questions in their new form: how do we know? what is it that we gain knowledge of? what principles must be accepted prior to scientific inquiry? and how may we justify their use? how may we gain knowledge of general principles when our every experience and every experiment is of single occurrences? which characteristics of the logical structure of a body of science are due to the particular sector of nature investigated by the special science involved and which are common to scientific inquiry as such? what is the nature of the warrant in Dewey's famous epitomization of scientific judgments as warranted assertions? These questions become an integral part of our course.

3. To relate the growth of scientific activities and the development of scientific knowledge to other human activities: those prior, contemporary, and subsequent to the scientific ones, and particularly the historical, biographical, sociological and economic connections of these scientific events.

Progress towards this goal achieves two ends at once. First, it makes clear that the concrete methods used by working scientists are not the dissected anatomical parts revealed by the analytic philosophers and indeed that the methods of scientists, working at even the most inclusive science, have been distinct from the implicit logic of science. Whether that logic should be made explicit, and its use made critical (and hence whether the methodology and the logic might be fused to achieve greater incisiveness of the scientific tools), is a discussion topic for our course based on case studies, and by no means a settled question. Second, this goal emphasizes that the scientist is a man like other men, prey to the same errors and prejudices, and heir to the same ideals and values. As a scientist he is thinking in terms which relate to his life as a whole and to his society as a continuing historical object. And the reciprocal nature of these relationships must be stressed in two ways: the dependence of science on society for its problems, for its substantial support, for its personnel, and possibly for its major categories of explanatory description; and the functional dependence of modern society, particularly since the commercial and industrial revolutions, on science for the content and the form, if not the spirit, of its daily life.

This fusion of goals entails, without doubt, a difficult course which requires many skills both of instructor and of students. And it is surely too demanding to be considered as a required generalization course for freshmen. At Wesleyan it is, rather, one of the culminating courses of a liberal education, elected by juniors and seniors (and, by permission, by a few sophomores of high standing). These upperclassmen have had a laboratory science course of the usual first-year type, presumably in the biological sciences; they have had a course in the humanities which is oriented about a modified great books program;[8] and a large fraction have had one or more courses in philosophy. About a third take an intensive course in intellectual history concurrently and this will be encouraged. Furthermore, the present course is not restricted to humanities and social science majors: pending the creation of a general course for senior science majors on the historical, philosophical and social relations of science these students are welcomed into the present one with the provision that they may be asked to do extra reading or writing. Their contributions to discussion have been important.

The students, then, are prepared in a general way for a mature treatment of the subject, more for the social and philosophical aspects then for the scientific content. This latter fact has led to the addition of a fourth goal.

4. To provide acquaintance with the fundamental ideas of mathematics (as far as elementary calculus) *both as a systematic logic and as applied, and as they appear along with the physical science.* This is basic to our over-all purpose; for example, no intuitive notion of velocity or acceleration

[8] A brief description of this course was given by N. O. Brown, "The Humanities at Wesleyan" in E. J. McGrath, ed., *The humanities in general education* (Wm. C. Brown Company, Dubuque, 1949). Concerning the stress on the humanities at Wesleyan, see the discussion by F. B. Millett, *The rebirth of liberal education* (Harcourt, Brace, New York, 1945).

will suffice to grasp the great difference between Aristotelian and Galilean physics. We must also clarify the two basic kinds of knowledge: the certainty of pure mathematics, and the probability and uncertainty of experimentally-verified science. For these no cultural summary of mathematics is enough; the actual mathematical notions are needed. One may limit the range of exposition and the breadth of application but one cannot afford to eliminate the rigorous content of the key notions and their application to the physical topics which are learned.[9]

The class is small; at no point thus far has it been greater than fifteen although it could be expanded easily to twice that number without revision of the present pedagogy. It is conducted largely by informal lectures, which the students are encouraged to interrupt. Aside from the texts and the problem exercises, additional readings are assigned in the large reserve.[10] Evidence of the extent and comprehensiveness of this reading was obtained in two ways: during the latter half of the fall semester, a long essay was required, topics being chosen from a list of fifty alternatives.[11] Throughout the spring semester, a set of specific reserve readings was assigned to all students, on which précis-critiques were written. Fifteen-minute quizzes were frequently given and hour examinations were set three times each semester. Neither demonstration lectures nor laboratory work have been feasible as yet. In a limited way, both will be added during 1950–51 with the aid of Professor V. E. Eaton's many skills in each.

In lectures, the fundamental ideas underlying the physical sciences and scientific methods are developed in their historical context and applied to significant problems, both classical and contemporary, one factor in selection being the unity of science. It has been useful in stimulating interest to stress occasionally the scientist's growing sense of power. Comprehension of Newton's great fusion of celestial and terrestrial physics, or the calculation of the age of the earth by inference reaching far beyond the experience of the entire race, not to speak of the individual scientist and his fellows, are instances producing awareness of such a sense.

A major period of about six weeks is devoted to one historical epoch; in 1949–1950 the period studied was the era in which Greek science developed. Because the Greek scientists were also natural philosophers, because they revealed so many basic epistemological and ontological problems, because Greek science was so intimately involved with Greek political history

[9] There is an ample supply of excellent books for this purpose. Until very recently, the most famous and perhaps the best of the introductions to fundamental mathematics has been A. N. Whitehead's *An introduction to mathematics* (Home University Library, Oxford University Press, London and New York; 1911 and 1948). New serious rivals are H. Cooley, D. Gans, M. Kline, and H. E. Wahlert, *Introduction to mathematics: A survey emphasizing mathematical ideas and their relations to other fields of knowledge* (Houghton Mifflin, rev. ed., Boston, 1950); E. C. Titchmarsh, *Mathematics for the general reader* (Hutchinson's University Library, London, 1950, and Longmans, Green, New York, 1950); E. P. Northrop *et al.*, *Fundamental mathematics* (University of Chicago Press, 3rd ed., Chicago, 1948). For library readings, see T. Dantzig, *Number, the language of science* (Macmillan, New York, 1935); L. Hogben, *Mathematics for the million* (Norton, rev. ed., New York, 1940); E. T. Bell, *The development of mathematics* (McGraw-Hill, rev. ed., New York, 1949); G. Polya, *How to solve it* (Princeton University Press, Princeton, 1945); E. Kasner and J. Newman, *Mathematics and the imagination* (Simon and Schuster, New York, 1940). For advanced students, the outstanding treatment is that of R. Courant and H. Robbins, *What is mathematics?* (Oxford University Press, New York, 1941). Lecturers can consult a vast literature, e.g., F. Klein, *Elementary mathematics from an advanced standpoint* (Original ed. Berlin, 1908; latest English ed., Dover, New York, 1945), C. Boyer, *The concepts of the calculus: A critical and historical discussion of the derivative and the integral* (Hafner, new ed., New York, 1949), R. B. Kershner and L. R. Wilcox, *The anatomy of mathematics* (Ronald Press, New York, 1950).

[10] The reserve list of 180 titles is divided into six sections: general physical science texts, mathematics, source material, historical, philosophical, social. Assigned texts during 1949–1950 were: B. Farrington, *Greek science I* and *II* (Penguin Books, Harmondsworth, England, 1944 and 1949, respectively); H. T. Pledge, *Science since 1500* (H. M. Stationery Office, London, 1939 and Philosophical Library, New York, 1946); I. Freeman, *Modern introductory physics* (McGraw-Hill, New York, 1949); J. B. Conant, *On understanding science* (Yale University Press, New Haven, 1947); A. N. Whitehead, *An introduction to mathematics* (Oxford University Press, New York, 1948); K. Krauskopf, *Fundamentals of physical science* (McGraw-Hill, New York, 2nd ed., 1948) for chemistry, geology, and astronomy; and A. Einstein and L. Infeld, *The evolution of physics* (Simon and Schuster, New York, 1938). During 1950–1951, the books by Pledge, Freeman, Conant, Whitehead and the astronomy section of Krauskopf, will be replaced, respectively, by: H. Butterfield, *The origins of modern science: 1500–1800* (G. Bell, London, 1949, and Macmillan, New York, 1950); G. J. Holton, *Methods and theories in physical science* (Addison-Wesley, Cambridge, lithoprint, 1950, published ed. 1951); J. B. Conant *et al.*, four *Harvard case histories in experimental science* (Harvard University Press, Cambridge, 1950); E. C. Titchmarsh, *Mathematics for the general reader* (Hutchinson, London, 1950 and Longmans, Green, New York, 1950); and L. Goldberg and L. Aller, *Atoms, stars and nebulae* (Blakiston, Philadelphia, 1943).

[11] Copies of the reserve library reading list, sample quizzes and examinations, and the list of essay topics are available from the author for those interested.

and social structure, because Greek scientific methods variously demonstrated pure empiricism, experimentalism, *a priori* rationalism, mathematical analysis (both pure and applied), postulational-deductive systems, and purely inductive systems, because the students have a bowing acquaintance with Greek philosophy, and not least because ample printed material is available, this period was chosen for detailed study and with concurrent attention to all four of our goals.[12]

Students were especially interested in Professor Farrington's beautifully written little paper-bound volumes, *Greek science I* and *II*, which were prepared by a thorough scholar who is at home in the life and thought of the ancient world, who has a profound and lively social sense, and who fully understands scientific method. Other periods might, of course, be chosen, among the more attractive being the age of Newton, the rise of atomic theory in chemistry, and modern nuclear physics. In each case, the study of the history of science is not undertaken for its own sake, but rather it is designed to further an understanding of the cultural setting which fosters the science of each age and of the impact science had on the rest of the society in question.[13]

Subject Matter of the Course

A few highlights from the syllabus may be of use in interpreting the difficulties and the values of this course.

1. Trigonometry is introduced by the story of Thales' use of similar triangles to measure the height of a pyramid. In order to discuss this fully, Babylonian mathematical tables based on empirical generalization, the Greek formulation of necessary proof, the elementary conception of a function, the enormous power and conciseness of trigonometry relative to geometry, are necessary topics for study and lecture.

2. Newtonian mechanics, necessarily a major portion of the syllabus, is given a brief one-lecture historical introduction and then, contrasting with the previous social and philosophical treatment of Greek science, care is taken to present classical mechanics in systematic and mathematical fashion. This leads to a discussion of the postulational-deductive character of theoretical physics, and then to the question of definitional or conventional elements in physical knowledge. This is a large and complex unit of the course: it involves more mathematics, it teaches the application of the Newtonian synthesis to other sci-

[12] In addition to some standard works on Greek history and philosophy, the following titles were placed on reserve as a working library in Greek science: M. Cohen and I. Drabkin, *A sourcebook in Greek science* (McGraw-Hill, New York, 1950); Lucretius, *On the nature of things* tr. Munro in *The stoic and epicurean philosophers* (Random House, New York, 1940); I. Thomas, tr., *Greek mathematical works* (vol. 335 and 362, Loeb Classical Library, Harvard University Press, Cambridge, 1939 and 1941); C. Bailey, *The Greek atomists and Epicurus* (Oxford University Press, Oxford, 1928); E. T. Bell, *The development of mathematics* (McGraw-Hill, rev. ed., New York, 1949); B. Farrington, *Greek science I* and *II* (Penguin Books, Harmondsworth, 1944 and 1949); B. Farrington, *Science in antiquity* (Home University Library, Oxford University Press, London and New York, 1947); B. Farrington, *Head and hand in ancient Greece* (Watts, London, 1947); B. Farrington, *Science and politics in the ancient world* (Allen and Unwin, London, 1946); T. L. Heath, *A manual of Greek mathematics* (Clarendon Press, Oxford, 1931); T. L. Heath, *A history of Greek mathematics* (Clarendon Press, Oxford, 1921); T. L. Heath, *Mathematics in Aristotle* (Clarendon Press, Oxford, 1950); J. L. Heiber, *Mathematics and physical science in classical antiquity* (Oxford University Press, London, 1922); W. A. Heidel, *The heroic age of science* (Williams and Wilkins, Baltimore, 1933); A. Reymond, *History of the sciences in Greco-Roman antiquity* tr. R. G. de Bray (Methuen, London, 1927); C. Singer, *A short history of science* (Clarendon Press, Oxford, 1941); C. Singer, *Greek biology and Greek medicine* (Oxford University Press, Oxford, 1922); F.S.C. Northrop, *The mathematical background and content of Greek philosophy* in *Philosophical essays for Alfred North Whitehead* (Longmans, Green, New York, 1936); H. Kelsen, *Society and nature* (Routledge and Kegan Paul, London, 1946 and University of Chicago Press, Chicago, 1939); F. M. Cornford, *Greek natural philosophy and modern science* and *The Marxist view of ancient philosophy* (a critique of Farrington) both reprinted in *The unwritten philosophy and other essays* (Cambridge University Press, Cambridge, 1950); F. Enriques, *The historic development of logic*, tr. J. Rosenthal (Holt, New York, 1929). The reader may consult, also, Otto Blüh, "Did the Greeks perform experiments?" *Am. J. Physics* **17**, 384 (1949), J. J. McCue, "Ancient science and the modern curriculum" *Am. J. Physics* **16**, 404 (1948), and H. Cherniss, *The riddle of the Academy* (U. of California Press, Berkeley, 1945).

[13] Whether lessons of the kind mentioned can in fact be learned from study of the history of science is an open question. Perhaps the scientific life can be taught and understood without an historical-factual approach; but it would seem that there are a number of characteristics of that life which are knowable only by means of the historian's approach, e.g., the less-than-crucial role of the logically "crucial experiment" in the actual acceptance or rejection of new scientific theories. Whether science be conceived as a chapter in the sociology of knowledge or as one in the philosophical foundations of culture, that conception, *to be itself a scientific one*, must be based on intersubjective data (i.e., the objective evidence of historical research) rather than the instructor's personal limited experience. A mature course should reflect this epistemological situation. In this connection, see the quotation from Max Black, *Am. J. Physics* **18**, 103 (1950), and H. Weyl's penetrating comments, *Philosophy of mathematics and natural science*, Part II, Ch. II, Para. 21, *Formation of theories* (Princeton University Press, Princeton, 1949).

entific fields (e.g., in 1950 this was the kinetic theory of gases), it raises the problem of absolute time and space via study of the introductory scholium to the *Principia*, it demonstrates various kinds of verification. Finally, in the concluding two weeks of the fall semester and following President Conant's example, it is used to exemplify and to examine critically the Marxist theory of historical materialism and economic determinism as applied to this particular epoch in the history of science.

3. Inorganic chemistry and light each receive about three weeks, the emphasis being placed on experimental procedures, the use of data, and the technique of inference. The discussion of light culminated last year in an elementary but intensive week on Huyghens' principle, while chemistry was ended by study of Mendeleef's table, and the two were joined by a discussion of line spectra.

4. Throughout the year, several case-studies were used, among them Mr. Conant's well-known organization of the material on Robert Boyle and the "spring of the air",[3] the rise and fall of the phlogiston theory, and Ernst Mach's reaction to his scientific and philosophical environment (the latter chiefly as exemplified by the use of hypothetical entities) as well as his subsequent influence. These were given no more than a week each but considerable preparatory reading was required.

5. Two concepts were stressed throughout the work of the second semester: fields and waves. Their recurrence in the explanatory description of several areas of science, their philosophically problematic meaning, and their deductive fertility when stated in appropriate mathematical language, make them particularly useful foci for our purposes. The work on electricity and magnetism stresses this rather than experimental or industrial aspects.

Obviously these stresses could have been laid on various topics other than the ones which actually received them during this past year. Electricity might well have been approached experimentally, and light theoretically; astronomy might have been made a study in method instead of one in cosmology; pure and applied mathematics might be treated as a necessary tool rather than as a study in the philosophic analysis of science. But at some time during each academic year, some historical period, not necessarily the Greek, will be explored in detail; at another time in each year a considerable body of theory will be examined as an axiomatized deductive system with attendant problems of experimental verification, practical application, and postulational meaning; a portion of the physical sciences will be treated as fundamentally inductive and experimental and as essentially reproducible by any student; some significant discoveries or errors will be analyzed as they occurred and as they painfully became accepted as matters of scientific fact; and throughout the course a connective tissue of mathematics, epistemology, and descriptive science will be supplied.

This fusion of the logic of science, the life of science, and the knowledge of science leads to enormous range and high selectivity. Despite this, the physical science course at Wesleyan seems to be moderately successful, perhaps because of the mature level of presentation and discussion, the prerequisites of philosophic reading and of a laboratory science, and also because of the refusal to lower standards because of the general educational nature of the course. Under these conditions, which might be realized at the upperclass level in many more colleges than first seems likely, it seems possible to combine modern man's crucial education in "the strategy and tactics of science" with his no less important learning of the knowledge gained by these scientific activities.

The religion of the Greeks never developed to the stage where it had a serious alternative view of the world to put forward which could conflict with the view to which they were led by common sense. They had no Bible or creed or powerful priesthood opposed to new ideas. Hence there was almost no persecution of the kind we shall meet with in the later pages of this book.—A. ARMITAGE, Sun Stand Thou Still (H. Schuman, New York, 1946).

NOTES AND DISCUSSION

Velocity of a Projectile by Direct Measurement; Resolution of Velocities

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THE author believes that the following experiment is instructive and suited to arouse interest at an early stage of the general physics course before the student has acquired theory adequate for most experiments.

In the first part of the experiment the student determines the velocity of a projectile by the simple measurement of horizontal distance traversed and time. The projectile is fired horizontally by the gun of a Blackwood Ballistic Pendulum.[1] The time during which the projectile is in flight is measured in 120ths of a second by a Cenco Impulse Counter[2] and a standard 60-cycle circuit reduced in voltage to 20 volts by an autotransformer. This circuit includes a rheostat, the counter and a suitably placed target. The counter is partially short-circuited through the shaft of the gun and another rheostat. On firing the gun this short-circuit is automatically opened, permitting the counter to operate until the projectile hits the target which breaks the main circuit through the counter.

The target, a light aluminum plate about 3 by 4 inches in size, is delicately suspended in a vertical plane between two wood strips clamped to an upright rod in a tripod base. Three $\frac{1}{4}$-inch metal rods inserted vertically in the wood strips form the supports for the target, one below and two above. The projecting ends are ground down to horizontal surfaces with shoulders on the side facing the gun to facilitate placing the target in position. The two upper rods are actually window bolts[3] having light compression springs which insure good electrical contact with the target yet permit it to be instantly released when struck by the projectile. To simplify the accurate measurement of the horizontal distance from the gun to the target, plumb lines are dropped from each.

The plumb line at the gun must be especially designed as it must straddle the base of the gun and its support assuming that the gun is mounted on a large tripod rather than on a table. This is accomplished by inserting in the plumb line two horizontal rods each longer than the width of the base of the gun to form a hollow rectangle encompassing the gun and its support. For reasons of stability and convenience the upper rod is really an inverted cross horizontally elongated, the vertical section of which terminates in a fine point at the lower end within the rectangle. In use, the plumb line suspension above this rectangle is so set that the terminal point is at the position where the projectile leaves the gun.

Though this experiment was designed to be given before the student has had the theory of the path of a projectile for determining the theoretical velocity, yet he will no doubt have had the formula $h=\frac{1}{2}gt^2$ for the height through which a body falls from rest during time t, from which he may solve for t and check the timer and thus determine the theoretical velocity since in this case the horizontal velocity is identical with the total velocity at the instant of firing. The especially designed plumb line greatly simplifies the determination of the height of the projectile on leaving the gun, while the establishing of the level at which the projectile hits the target is accomplished by use of an indicator mounted on an adjacent tripod.

In the second part of the experiment the gun with its support is tilted through an angle of 30 degrees or more, necessitating a shift in the position of the target. The experimental results now give only the horizontal component v_x of the total velocity, which should be checked against the theoretical value $v_x=v\cos\theta$, where θ is the angle of elevation of the gun, and v is the total initial velocity as determined in the first part of the experiment.

Of course when the gun is fired at an elevated angle the total velocity is somewhat changed. However, remembering the formula $v^2=2as$ for the final velocity of a body starting from rest with acceleration a through a distance s, and noting that the shaft of the gun moves a total distance of only about 4 cm with a final velocity of about 600 cm/sec, the average acceleration is of the order of 45,000 cm/sec^2, so that the change of initial velocity as a result of tilting is of the order of 1 percent.

Of the various ways for determining the angle of elevation, we prefer having the student sight along a straight-edge parallel with the gun which may conveniently be the support on which the gun is mounted. He finds the point on the floor which lies on a continuation of this line, and dropping a plumb line from the straight-edge he has a right-angled triangle whose altitude and base he can measure with good accuracy thus establishing the tangent of the angle of elevation θ.

[1] Central Scientific Company, Chicago, Illinois, Cat. No. 75425.
[2] Central Scientific Company, Cat. No. 73506.
[3] Window bolts are available at any hardware store for fifteen cents per pair.

The Calculation of Work in Elementary Thermodynamics

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THE concept of "net" or "practical" work as distinct from reversible thermodynamic work is probably quite familiar to chemists and engineers. Talks with physicists about an elementary problem—treated later as an example—suggested to us that many might welcome a short note on this topic. Only one or two books that we know mention the subject and of these the only one[1] in

which it is fundamental to the argument seems to us misleading. However, the same author in his *Separation of Gases*[3] gives a clearer but still imperfect discussion of the problem.

Thermodynamic work.—The first law is usually written as $dU = đQ + đW$, where dU is the increase in the internal energy of the system and $đQ$ the heat entering it. The work $đW$ is that done *on* the system and will for reversible changes generally include terms of the type $-pdv$, γdA, HdM, etc., where p and v are, respectively, the pressure and volume; γ and A the surface tension and surface area; and H and M the magnetic field and magnetic moment. Here we take the system to be only the fluid, the surface and so forth and do not include the devices we may use actually to achieve the work. Thus the quantity dv refers to the total volume change of the fluid. This may or may not be equal to the volume change produced by the piston displacements.

The total work done in a specified change will be $\Delta W = -\int pdv + \int \gamma dA \cdots$ and this quantity will be called the *thermodynamic work.*

Net work.—In most actual cases the mechanism doing the work will be helped or hindered by the atmosphere. If the total volume as seen from the atmosphere is v' then the work done against the atmosphere will be $\int p_0 dv' = p_0 \Delta v'$, so that the total practical work needed to achieve the contemplated change will be the sum of this term and the thermodynamic work already defined. Thus we have

$$\Delta W' = \Delta W + p_0 \Delta v'. \tag{1}$$

We call this the *net work.* In practice, once work is done on the atmosphere it cannot be recovered so that if we wanted to eliminate the term $p_0 \Delta v'$ we should have to adopt some trick such as backing the pistons with a vacuum. The net work can be calculated directly without actually evaluating the thermodynamic work. This point will be illustrated in the first of two examples which we now discuss.

First example: Work required to expand a soap bubble.—A question taken from an Oxford examination paper reads: "A soap bubble of radius R_1 and surface tension γ is expanded at constant temperature by forcing in air. How much work has to be done to increase the radius to R_2?" For definiteness we can suppose the process to be as shown in Fig. 1.

We shall first calculate the thermodynamic work—this must be a unique quantity since the process is isothermal—and then by direct integration find the net work, thus verifying that Eq. (1) holds good.

Thermodynamic work. First method.

$$\Delta W = \Delta U - \Delta Q.$$

Now $\Delta U_{\text{gas}} = 0$, $\Delta U_{\text{surface}} = 2(\gamma - T(\partial\gamma/\partial T)_A)\Delta A$.

$\Delta Q_{\text{gas}} = -p_2 v_2 \ln(p_2/p_1)$, $\Delta Q_{\text{surface}} = -2T(\partial\gamma/\partial T)_A \Delta A$.

Hence

$$\Delta W = p_2 v_2 \ln(p_2/p_1) + 8\pi\gamma(R_2^2 - R_1^2).$$

Second Method.

$$\Delta W = -\int_{V_1+v_1}^{v_2} p\,d(V+v) + \int_S \gamma dA$$
$$= p_2 v_2 \ln(p_2/p_1) + 8\pi\gamma(R_2^2 - R_1^2),$$

as before.

Net Work: Instead of using Eq. (1) we write down the actual work that would be done by the piston. This gives

$$\Delta W' = -\int_{V_1}^{0} (p - p_0) dV.$$

(Notice that, in accordance with Fig. 1b, dV refers to the piston displacements only.) Using the equations $p - p_0 = 4\gamma/R$, $p(v+V) = p_2 v_2$, we find

$$\Delta W' = \Delta W + p_0(v_2 - v_1 - V_1).$$

Since in this case $\Delta v' = v_2 - (v_1 + V_1)$, formula (1) is directly verified.

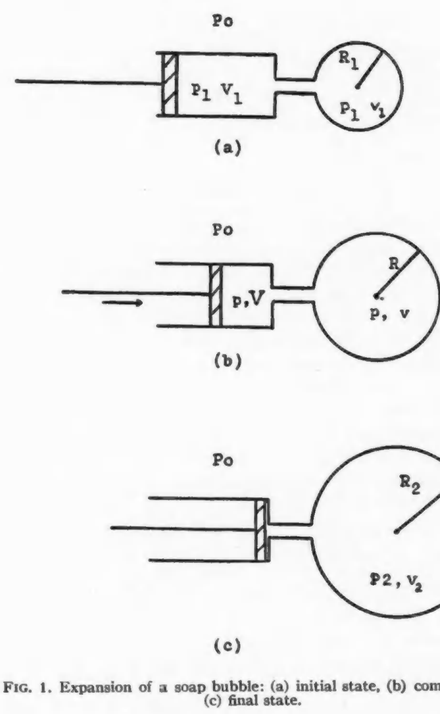

FIG. 1. Expansion of a soap bubble: (a) initial state, (b) compression, (c) final state.

Second example: Minimum work for liquefaction.—The problem here is to find the minimum work necessary to transform gas at p_0, T_1 (state 1) into liquid at p_0, T_2 (state 2), where p_0 is the atmospheric pressure.

The first law $\Delta U = \Delta Q + \Delta W$ can be written as

$$\Delta H = \Delta Q + \Delta W + \Delta(pV)$$
$$= \Delta Q + \Delta W + p_0 \Delta V,$$

where $H=U+pV$ is the enthalpy of the system. That is,

$$\Delta H=\Delta Q+\Delta W'. \tag{2}$$

By arguments based on the second law it can be shown that $\Delta Q\leq T_0\Delta S$, where T_0 is the temperature of the lowest available reservoir, and S is the entropy. Therefore

$$\Delta W'\geq\Delta H-T_0\Delta S.$$

This is the usual formula. Notice that its validity depends on the fact that the initial and final pressures are both taken to be p_0. Of course, this condition is frequently satisfied in other contexts and, whenever it is, Eq. (2) can be used instead of the first law. Incidentally, the fact that we can calculate the minimum work for a process does not necessarily mean that it can be carried out, even in principle. The third law says that it is impossible to reach absolute zero although $\Delta W'_{\min}$ for such a process is finite.

[1] Ruhemann, *Low temperature physics* (Cambridge University Press, 1937), p. 14.
[2] Ruhemann, *Separation of gases* (Oxford University Press, 1949), 2nd edition, p. 110.

AMERICAN JOURNAL OF PHYSICS VOLUME 18, NUMBER 9 DECEMBER, 1950

LETTERS TO THE EDITOR

Abstracting and Indexing Services of Physics Interest

SINCE the publication, several months ago, of the subject list[1] prepared during the Study of Physics Abstracting conducted by the American Institute of Physics, several additions and corrections have been received. They are as follows:

Added Entries

146. Jernkontorets Litteraturöversikt

Description: This survey of literature from The Metal Agency in Sweden abstracts papers in the various fields of metals interest. Abstracts appear under headings such as Science of Mining, Heating, Metallurgy, Forging Techniques, Heat Treatment and Preparation, Metallography, Nature of Metals and Alloys, Corrosion and Corrosion Protection, and Testing of Materials.

Magnitude (approx.): 1000 abstracts per year.

Publication Information: Jernkontorets Litteraturöversikt is published monthly by Jernkontorets Litteraturtjänst (The Metal Agency's Literature Service), Kungsträdgardsgatan 6, Stockholm C, Sweden. Subscription price is approximately $2.50 per year.

147. Mekanförbundets Litteraturöversikt

Description: Covers Swedish and foreign journals, abstracting papers of special interest to the mechanical industries. Abstracts also are available in sheets for pasting on 75 mm×125 mm library cards.

Magnitude (approx.): Over 1000 abstracts per year; 70–80 journals.

Publication Information: Mekanförbundets Litteraturöversikt is sponsored by the Association of Swedish Mechanical Industries and can be purchased from Sveriges Mekanförbund, Malmtorgatan 10, Stockholm, Sweden, at 70 Swedish crowns ($14) per year.

148. Aeronautical Documentation (Netherlands)

Description: This Netherlands central documentation service of aeronautical literature is issued on cards giving information regarding source of paper, scope of paper, indexing data and a brief abstract usually in the same language as the paper. Fields covered include aeronautics, applied mathematics, mechanics, physics, materials, and instruments. All periodical and nonperiodical literature available at the institutions which participate in the National Aeronautical Institute (Amsterdam) are covered. Cards are issued within 2 to 3 months after paper is received in library.

Magnitude (approx.): Some 12,000 cards anticipated during 1950; 150 journals and over 40 nonperiodical series of papers.

Index: Being in card form, service is itself an index as well as a collection of abstracts.

Publication Information: Service issues from Supervisory Committee for Aeronautical Documentation, National Luchtvaartlaboratorium, Sloterweg 145, Amsterdam, Holland. Subscription rate varies from year to year but for 1950 was set at 750 Dutch florins. Further information can be obtained from the Secretary of the Supervisory Committee at the address given above.

149. Literature of Applied Spectroscopy

Description: Service lists literature in the field of applied spectroscopy appearing in U. S. and foreign publications. No abstracts are given.

Magnitude (approx.): 150–200 papers listed per issue.

Publication Information: "Literature of Applied Spectroscopy" appears as a section of the *Spectroscopy Bulletin*, published quarterly by the Society for Applied Spectroscopy. At present the *Bulletin* will be sent gratis to any library or technical institution requesting it; requests should be addressed to G. L. Buc, Editor, Society for Applied Spectroscopy, American Cyanamid Company, Calco Chemical Division, Bound Brook, New Jersey.

150. Elektrotechnische Berichte

Description: Service is selective of articles of significance in the field of electronics. Abstracts are informative and written by subject experts; rarely is any critical review of papers included. Books and patents also are covered.

Magnitude (approx.): 600 abstracts per year; 100–150 journals.

Publication Information: Elektrotechnische Berichte is published approximately three times per year by Dozentur für Geoelektrik, Vienna IV, Gusshausstr. 25, Austria. Information regarding subscription rates should be obtained from the publisher.

Corrections and Revisions

79. Métallurgie, Revue de

We are informed by the editors of this journal that the original entry was in error in giving the Société Francaise de Métallurgie as the publication's publisher. The journal itself should have been so shown; however, the street address as originally stated is correct. *La Revue de Métallurgie*, in addition to carrying a department of abstracts in its field, publishes the papers presented at the annual and monthly meetings of the above-named society as well as results of the research conducted at the Institut de Recherches de la Sidérurgie Francaise (I.R.S.I.D.).

86. Mining and Metallurgy, Bulletin of the Institution of

Abstracting section of the *Bulletin*, formerly called "Index of Recent Articles," has been replaced by "I.M.M. Abstracts." The new department is a world survey of literature on economic geology and mining (except coal), mineral dressing, extraction metallurgy (excluding iron) and allied subjects. Annual subject and author indexes will be issued. Magnitude will be similar but increasing. Price of *Bulletin* remains the same.

137. Textile Research Journal

Beginning with 1950 the former abstract department of this journal was discontinued and the Abstract Section of the British Journal of the Textile Institute (see List entry No. 136) substituted for it. Bound copies of this section of the British journal are included in subscriptions to the *Textile Research Journal*.

145. Meteorologische Rundschau

When the entry for this journal was first published, subscription price information was not available. Cost of volumes and parts thereof still available are as follows:

1947–48	Vol. 1, Nos. 7–18	$11.42
1949	Vol. 2, Nos. 1–12	$12.85
1950	Vol. 3, Nos. 1–6 complete	$14.28.

Numbers 1–6 of Volume 1 (1947) are out of print.

DWIGHT E. GRAY

Library of Congress, Navy Research Section
Washington 25, D. C.

[1] *Am. J. Physics* **18**, 274–299 (1950). The list also is available for 75 cents from the Office of Technical Services, U. S. Department of Commerce, Washington 25, D. C.; serial number of the publication is PB99951.

The Rope Trick: Energy *vs.* Momentum Method

PROFESSOR Stephenson[1] has added a note to many by others on this problem which is essentially this: Find the force required for a sailor walking with a velocity v to pull a rope with mass m_1 per unit length along a frictionless floor from a heap of the rope. By the momentum–impulse method it is m_1v^2; by the usual kinetic energy–work method it is $m_1v^2/2$. The former is the correct answer. The usual explanation involves replacing the rope with a chain, when it can be seen that each link starts out with too much kinetic energy, part of which is frittered away in heat by bouncing back and forth at first.

However, there is a much more elegant solution. The usual expression $mv^2/2$ for kinetic energy is derived from $\int fds=\int[d(mv)/dt]ds=\int mvdv=mv^2/2$, if the mass is kept constant. This rope case, though, can be considered a beautiful example of the case where the mass increases from zero to m and the velocity is kept constant. Imagine each part of the rope nonexistent until it suddenly has a velocity v. Then the kinetic energy is $\int fds=\int[d(mv)/dt]ds =\int_0^m v^2dm=mv^2$. If mv^2 is used instead of $mv^2/2$ in the kinetic energy–work method, the rope force becomes m_1v^2 which is correct, with no qualifications needed. The internal problem of link oscillation does not arise here as all existing components have at all times the same velocity v.

EDWARD M. LITTLE

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[1] R. J. Stephenson, *Am. J. Physics* **17**, 224 (1949).

Preparing Rods for Stroking in the Kundt's Tube Experiment

AFTER a cleansing rub with ethanol, an end section of a rod can be quickly coated with a hard film of rosin by a simple dipping-and-drying process: The dipping mixture (200 grams of rosin per liter of ethanol) is stocked ready for use in a tall bottle with an auxiliary supply for replacements, because rosin mixes slowly at such concentration. A fresh dip is partially dried (not burned) over a flame, the rod being horizontal and rotated. When most of the solvent has been vaporized, the drying is completed by careful passes through the flame. The sticky warm film is hardened by cooling (water). For greater hardness (tested by gently stroking), the cured film is reflamed. For the longer rods, the coated sections are 25 cm long. About three to six minutes are required, the time depending on the coated area.

When cracked by vibrations during a few conditioning strokes, the hard film yields an evenly distributed supply of rosin dust for many strokes with a chamois patch or the fingers. The vibration amplitudes are easily controlled by the vigor of the stroking.

BERNARD L. BRINKER, O.S.B.

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The Electric and Gravitational Proportionality Constants

IN a recent letter[1] J. S. Miller notes that the energy density in the gravitational field $\mathbf{g}$ is $\mathbf{g}^2/\pi G$ as compared with the terms $\epsilon\mathbf{E}^2/8\pi$ and $\mu\mathbf{H}^2/8\pi$ for the energy of the electromagnetic field; he asks why the proportionality constant G occurs in the denominator while ϵ and μ are in the numerator? The answer is that the constants have been so defined. However, I would not discuss the question so

curtly. The question itself implies that one might expect a certain parallelism in our treatment of various fields; as a matter of fact, within the conventional formulation of electromagnetic theory there is a remarkable inversion of concepts which hides the essential relations between electricity and magnetism.

(1). The form of the energy-density relation depends upon the variables employed. The electrostatic term can be expressed as $\mathbf{D}^2/8\pi\epsilon$, the magnetic term as $\mathbf{B}^2/8\pi\mu$, and the gravitational term as $Gg'^2/8\pi$ where $g'=m/r^2=g/G$ (analogous to $D=q/r^2=E\epsilon$). However, for consistency one should in each case introduce the *field of force*, E, B, or g, defined by the relations $\mathbf{F}=q\mathbf{E}$, $\mathbf{F}=i\mathbf{l}\times\mathbf{B}$, or $\mathbf{F}=m\mathbf{g}$. The comparable energy density terms are then

$$\epsilon\mathbf{E}^2/8\pi,\quad \mathbf{B}^2/8\pi\mu,\quad \mathbf{g}^2/8\pi G.$$

But why do we usually prefer **H** *instead of* **B** *as the magnetic field variable?* The preference is so strong that the force relation is often given as $\mathbf{F}=i\mathbf{l}\times\mathbf{H}$, which is definitely wrong except when $\mu=1$. The quantity **H** is even called the "field of force."

(2). Constants ϵ and μ are properties of the medium, while the gravitational constant G is a universal constant, with a value depending on the units used, not upon the buoyancy of the surrounding medium. To resolve the discussion into its elements we should separate the absolute permeability μ into two factors $\mu=K\mu_r$, where μ_r, the relative permeability of the medium is the dimensionless quantity given in tables and K (sometimes called the permeability of space) is like G, a universal constant which depends upon the system of units. Similarly we should express ϵ as $\epsilon_r\epsilon_0$ or ϵ_r/k, where ϵ_0 or k is a universal constant.[2] Miller is presumably considering charges, currents and masses in empty space, where μ and ϵ reduce to the universal constants. For the repulsion between point charges

$$F=\frac{kq_1q_2}{r^2}(=Eq_2);$$

for the attraction between parallel-current elements which are "abreast" (perpendicular to the line of separation)

$$F=\frac{Ki_1dl_1i_2dl_2}{r^2}(=Bi_2dl_2);$$

and for the attraction between point masses

$$F=\frac{Gm_1m_2}{r^2}(=gm_2),$$

where G is the gravitational constant. k may be called the electrostatic constant, and K the magnetic constant ($K=k/c^2$, where c is the velocity of light). (One may prefer to regard the two fundamental universal electromagnetic constants as k and c rather than k and K). In any electrostatic system of units (for example, the centimeter-gram-second-statcoulomb system) $k=1$; in an electromagnetic system $k=c^2$ and in the mks coulomb system $k=9\times10^9$. We have written the magnetic and gravitational equations in conventional form but the first equation is usually expressed as $F=q_1q_2/\epsilon_0r^2$. *Why should Coulomb's law be expressed with the proportionality constant in the denominator?* The inconsistency in form is carried over into every equation in which the constants appear. For example, the subordinate fields in space are defined from the force fields by the relations $D=\epsilon_0E$ and $H=B/K$. Coulomb's law itself is complicated by the convention. All other conventional charge units are very large in comparison with stat-units and it is evident that with such large units the force will be increased by a large factor k; it is less clear to say that with large units the force is divided by a small fraction ϵ_0, to make the right side of Coulomb's law a compound fraction. If the constants are defined symmetrically, they appear symmetrically in the expressions for the energy density in free space

$$E^2/8\pi k,\quad B^2/8\pi K,\quad g^2/8\pi G.$$

(3). Now let us consider charges, currents and masses submerged in a liquid. A charge q_c induces a polarization charge q_p of opposite sign, a current i_c induces an "amperian current" i_m usually of opposite sign (diamagnetism), a mass m_c displaces a mass of liquid m_l and the resultant forces are, in general, determined by the action of the field on the resultant charge q_c+q_p, current i_c+i_p, or mass m_c-m_l.[3] If the liquid extends to infinity the force between charges is simply reduced a factor $1/\epsilon_r$, the force between currents by a factor μ_r and if all bodies have the same density the force between them is reduced by the factor γ, where

$$\frac{1}{\epsilon_r}=\beta_r=\frac{q_c+q_p}{q_c},\quad \mu_r=\frac{i_c+i_p}{i_c},\quad \gamma=\frac{m_c-m_l}{m_c}.$$

Quantity β_r, analogous to μ_r, may be called the "electric permeability." If we introduce a gravitational field $\mathbf{g}''$, which like **E** and **B** is defined in terms of the resultant force in the liquid ($\mathbf{g}''=\gamma\mathbf{g}$), the expressions for energy density become

$$\frac{\mathbf{E}^2}{8\pi k\beta_r},\quad \frac{\mathbf{E}''}{8\pi K\mu_r},\quad \frac{\mathbf{g}''^2}{8\pi G\gamma}.$$

Conventionally, the constant ϵ_r is introduced instead of β_r and the symmetry disappears. We are not interested in this forced analogy between buoyancy and the effect of a polarized medium. But *why do we express dielectric properties in terms of the dielectric constant instead of the "electric permeability"?*

The answer to all of our questions is found in the fact that historically the magnetic pole has been introduced as a "fictitious magnetic charge" and the inverted concepts have been introduced to force the fictitious analogy between a pole and a charge. To complete the perversion we have adopted the Kennelly rather than the Stratton-Sommerfeld definition of pole: $m_K=m_{SS}K$, so that $F=mH$ rather than $F=m\mathbf{B}$, and the meaning of the magnetic field of force is completely confused. When should we use gauss and when should we use oersteds, when B and when H? The answer is that B is the force field which acts on a current, an amperian current and a rationally defined pole. Rationally, H is the inducing field. Irrationally, we may define it as the "fictitious force field" which acts on a "fictitious magnetic charge."

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[1] *Am. J. Physics* **18**, 237 (1950).
[2] Coulomb's Law Committee Report, *Am. J. Physics* **18**, 1 and 69 (1950).

Teaching Alternating Current Circuits

In a recent letter to the editor by Saby[1] it is said that the use of vectors in an article entitled "Teaching Alternating Current Circuits"[2] was such as to give erroneous impressions since the limitations of the notation had not been sufficiently clearly expressed.

In considering whether or not alternating current and voltage are to be recognized as true vectors in the same sense that force, displacement, and momentum are true vectors, we may refer to the definition of a vector physical quantity. A vector physical quantity is defined as any physical quantity which has magnitude and direction and adds by the polygon method. If we accept this as a necessary and sufficient specification of a vector quantity, then alternating voltage and alternating current satisfy this definition to exactly the same degree as length, force, acceleration, momentum, or any other vector quantity. All of the rules which apply to vector quantities should therefore apply to alternating current and alternating voltage. One should therefore be able to add, subtract, multiply, and divide alternating current and voltage in exactly the same manner as done with other vector quantities. The process of addition and subtraction is easily recognized to apply equally well to alternating current and voltage as well as to other vector quantities. The process of multiplication to give scalar and vector products also applies to alternating current and voltage. For any impedance in a series a.c. circuit, voltage and current may be represented by vectors at an angle to each other equal to the phase difference between them. The scalar product of current and voltage equals the scalar quantity power delivered to the impedance. The vector product of current and voltage is something which has not yet been found of special value in describing alternating current circuits.

The division of one vector by another which is parallel to it is a concept more generally utilized in elementary physics than is often realized. Such division produces a scalar quantity as in the definitions: pressure $p=\mathbf{F}/\mathbf{A}$; compression or stretching stress $=\mathbf{F}/\mathbf{A}$, where $\mathbf{F}=$force on area $\mathbf{A}$; force constant of spring $k=\mathbf{F}/\mathbf{x}$, where $\mathbf{x}=$displacement; and mass $m=\mathbf{F}/\mathbf{a}$, where $\mathbf{a}=$acceleration produced by resultant force $\mathbf{F}$. The division of parallel vectors is equivalent to a scalar product and may be defined by $\mathbf{d}/\mathbf{b}=\mathbf{d}\cdot\mathbf{b}/\mathbf{b}^2$. The definitions of pressure, force constant, stress, and mass contain divisions of parallel vectors. The division of two vectors perpendicular to each other is equivalent to a vector product and may be defined by $\mathbf{d}/\mathbf{b}=\mathbf{d}\times\mathbf{b}/\mathbf{b}^2$. Two physical quantities defined by division of vectors perpendicular to each other are surface tension and shearing stress. Surface tension $=\mathbf{F}/\mathbf{L}$, where $\mathbf{F}$ is the force along a line of length $\mathbf{L}$ with $\mathbf{F}$ perpendicular to $\mathbf{L}$. Shearing stress $=\mathbf{F}/\mathbf{A}$, where $\mathbf{F}$ is the force distributed over area $\mathbf{A}$ and $\mathbf{F}$ is perpendicular to $\mathbf{A}$. Surface tension may be considered a vector perpendicular to the surface and shearing stress a vector lying in the surface.

The general case of division of one vector by another in any direction might be defined though it has not been used in vector analysis or descriptions of physics. The division of alternating voltage by alternating current to obtain impedance is neither a division of parallel vectors nor a division of perpendicular vectors. As pointed out by Saby it may be well to recognize that the definition of impedance $Z=\mathbf{V}/\mathbf{I}$ is a relation of magnitudes only. It could be written $Z=|\mathbf{V}|/|\mathbf{I}|$ or $|\mathbf{V}|=Z|\mathbf{I}|$. This does not mean that Z should be considered as a vector in the same sense that $\mathbf{V}$ and $\mathbf{I}$ are vectors. Impedance Z may be considered as a scalar multiplier. In deriving the equation for impedances in series one could write

$$\mathbf{V}=\mathbf{V}_1+\mathbf{V}_2+\mathbf{V}_3$$
$$|\mathbf{V}|\mathbf{v}=|\mathbf{V}_1|\mathbf{v}_1+|\mathbf{V}_2|\mathbf{v}_2+|\mathbf{V}_3|\mathbf{v}_3,$$

where $\mathbf{v}$, $\mathbf{v}_1$, $\mathbf{v}_2$, $\mathbf{v}_3$ are unit voltage vectors. Replacing $|\mathbf{V}|$ by $Z\ |\mathbf{I}|$ gives $Z\ |\mathbf{I}|\mathbf{v}=Z_1|\mathbf{I}|\mathbf{v}_1+Z_2|\mathbf{I}|\mathbf{v}_2+Z_3|\mathbf{I}|\mathbf{v}_3$. Cancelling $|\mathbf{I}|$ leaves $Z\mathbf{v}=Z_1\mathbf{v}_1+Z_2\mathbf{v}_2+Z_3\mathbf{v}_3$. If now Z, though not a vector, is treated as a vector in the direction of $\mathbf{v}$, the unit vectors do not need to be written and there results $Z=Z_1+Z_2+Z_3$. A similar discussion applies for impedances in parallel.

If desired, Z may be considered as an operator which acts on a vector in one direction to produce a vector of different magnitude in another direction. In this case, $\mathbf{V}=Z\mathbf{I}$ and $\mathbf{V}=\mathbf{V}_1+\mathbf{V}_2+\mathbf{V}_3$ may be replaced by $Z\mathbf{I}=Z_1\mathbf{I}+Z_2\mathbf{I}+Z_3\mathbf{I}$. The magnitudes of $\mathbf{I}$ may be cancelled to leave $Z=Z_1+Z_2+Z_3$. This may be treated as an equation relating vectors in the directions of the potential differences.

It may be of interest to point out that the use of vector additions in describing alternating currents and voltages is entirely equivalent to the use of vector additions in describing interference and diffraction of waves. The addition of vectors to obtain the resultant current through the generator in a circuit containing several impedances in parallel is equivalent to the addition of amplitudes for describing interference or diffraction. The resultant current has an amplitude determined by interference of the currents through the various impedances. In the same way the resultant voltage across a number of impedances in series is determined by the interference of the voltages across the separate impedances.

It appears, therefore, that alternating current and voltage may be treated as ordinary vectors with no violation of fundamental procedures of vector analysis. For the purposes of simplification, impedances may be added as vectors though by the above definition impedance is a ratio of magnitudes, and not a vector.

The introduction of conductance and admittance in elementary general physics inserts two additional physical quantities, and corresponding additional sources of confusion for students. It seems preferable to describe properties of electrical circuits in terms of only current, potential difference, and impedance and expect the students to add fractions correctly rather than introduce a new physical quantity to avoid the addition of fractions. It is sufficiently difficult to teach the meaning of impedance and the difficulty will be increased if admittance is also introduced.

It seems also that for students who have had only one course in high school algebra and one in high school geometry three or four years previous to their study of

physics, the use of $i^2=-1$ would cause unnecessary confusion.

It also does not seem desirable to add confusion by substituting words like phase diagram, or impedance diagram for vector diagram in describing alternating currents. It is hard enough to teach the vector method of addition without the complication of various kinds of vector diagrams for different kinds of vectors. Students can learn to use vector addition in describing forces in equilibrium; projectile motion; simple vibratory motion; alternating currents and voltages; and interference, diffraction, polarization, and intensity of waves if the same rules apply in all cases. After learning the use of vector addition to describe these experiments, students should be well prepared to learn vector multiplication in later courses.

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[1] John S. Saby, *Am. J. Physics* **18**, 321 (1950).
[2] Winans, Cole, Walters, and Hummel, *Am. J. Physics* **17**, 503 (1949).

Teaching A. C. Circuits—A Rejoinder

IN the above letter to the editor it is argued that because alternating current and voltage are completely specified by a magnitude and a direction, they should therefore be regarded as physical vector quantities.[1]

Several objections may be raised to this conclusion. The "direction" used in specifying circuit quantities is really not a physical direction, but merely a mathematical inverse trigonometric function, the so-called "phase angle." It is a direction only in mathematical space or on an arbitrary diagram. It is not a direction in physical space. Furthermore, it is a single angle, whereas two angles are needed to specify the direction of a physical vector quantity. This is one distinction between circuit quantities and physical vector quantities.

It will be noted that the complete physical specification of circuit quantities such as voltage actually requires more information than magnitude and "direction"—we must also know the frequency! This is in further contrast to ordinary physical vector quantities.

Mathematically, a.c. circuit quantities can be represented by complex numbers, and the relations among them, including "Ohm's law" can be expressed by algebraic equations using complex variables. On the other hand, physical vector quantities cannot be represented by complex numbers. They can be represented by vectors, but the relations among vectors are not those of complex algebra. The differences in mathematical representation between circuit quantities and vector quantities can hardly be ignored.

It is true that a.c. circuit quantities can be represented by directed line segments on a plane diagram. They have this useful property in common with physical vector quantities (provided the latter are coplanar). However, in the view of the above differences, we should help our elementary students to avoid misconceptions by using vector terminology with extreme caution. A qualifying statement similar to the following may be helpful: "Alternating current circuit quantities such as voltage, current, and impedance can be added like ordinary vector quantities when suitable direction conventions have been adopted, and each may be referred to as a vector quantity for the purpose of addition to like quantities, but the relations involving different quantities such as 'Ohm's law' are not vector relations."[2]

Students taught by the method[3] referred to in the above letter may also be confused by the fact that voltage and current are there regarded as physical vector quantities because they are "completely specified by a magnitude and a direction," while on the other hand impedance, which can be completely specified in exactly similar manner is variously referred to as "not a vector," "a scalar multiplier," and "an operator which acts on a vector in one direction to produce a vector of different magnitude in another direction." The latter refers to $V=ZI$, which is not a vector equation, anyhow, and none of the quantities in it should here be regarded as a vector quantity. There is no need to treat Z any differently from V and I—each may be added vectorially to like quantities.

With regard to "vector division" as described in the above letter, the concept is meaningless as purporting to be an inverse of the process of obtaining vector products. The algebraic equation $ab=c$ can be solved uniquely for b, given a and c, and we call the process division. On the other hand, there is no unique solution $\mathbf{b}$ for the vector equation $\mathbf{a}\times\mathbf{b}=\mathbf{c}$, given arbitrary vectors $\mathbf{a}$ and $\mathbf{c}$, nor for the vector equation $\mathbf{a}\cdot\mathbf{b}=c$, given $\mathbf{a}$ and c. An infinite number of possible solutions satisfies each equation. Vector division in this sense, therefore, does not exist. There is no law against giving that name to an arbitrary vector operation, but its meaning would be obscure.

The alleged physical examples of vector division cited can all be treated by conventional methods. There is no need to regard any of these examples as a vector division. In fact stresses may be more properly regarded[4] as examples of physical tensor quantities.

In conclusion, we may sometimes find colloquial use of vector terminology helpful in elementary teaching, but fundamental concepts will be obscured unless we use it correctly and within clearly expressed limitations.

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[1] Actually, these conditions are necessary, but not sufficient—for example: a finite rotation about an arbitrary axis can be uniquely represented by a magnitude and a direction, yet finite rotations cannot be added like vectors.
[2] J. S. Saby, *Am. J. Physics* **18**, 321 (1950).
[3] Winans, Cole, Walters, and Hummel, *Am. J. Physics* **17**, 503 (1949).
[4] Merit Scott, *Mechanics, statics and dynamics* (McGraw-Hill, 1949), Chap. 9.

ANNOUNCEMENTS AND NEWS

Book Reviews

Some Early Tools of American Science. I. BERNARD COHEN. Pp. 201+xxi. Plates 67. 15×23½ cm. Harvard University Press, Cambridge, 1950. Price $4.75.

This excellent book was written "to celebrate the first comprehensive exhibition of early scientific instruments at Harvard, and to provide some background for an appreciation of the larger values associated with these tools of early American science." As an illustrated catalog of the exhibition it demonstrates that Harvard's collections of "philosophical apparatus" during the 18th and early 19th centuries were probably the best in America and equal to those in many European colleges. It is, however, much more than an illustrated catalog of an interesting antiquarian exhibit for "in giving the background of the instruments—the way in which they were obtained, their use in teaching and research, and their preservation"—the author has made an important contribution to the social and intellectual history of this country.

The reason why this is an important book well deserving the attention not only of science teachers but of students of American cultural history is that it clearly documents the fact that, contrary to the statements made in most history books, science played a really important role in the early cultural development of this country. For Dr. Cohen shows that "Harvard and her early rivals were well abreast of the science of their day; that the scientific attitude of mind was inculcated in their students who had ample apparatus for experiments; that some of the professors organized scientific expeditions and made positive contributions to knowledge"; that "the ideals of investigation, observation, and publication of results which characterize the modern university were already present in Cambridge in the 18th century"; and that all the sciences, instead of being antagonistic to organized religion, were conceived and taught "not merely as areas of exact knowledge but as handmaidens of theology and moral philosophy."

The science student will also be entertained and instructed by some of the revealing accounts of the personalities of the early teachers at Harvard, of the memorable experiments performed by them—such as the first demonstrations of dry ice in this country—and of the problems connected with the maintenance of the instruments provided for their use. Thus Oliver Wendell Holmes' description of John W. Webster as "pleasant in the Lecture room, rather nervous and exciteable, I should say, and judiciously self-conservative when an explosion was part of the program" is illustrated by an amusing account of his "volcano" experiment; and the story of how the famous clock-maker, Simon Willard, succeeded in making the Pope orrery work after every one else had failed will delight every physicist who has ever faced a similar problem.

The first hundred and thirty pages of the book are devoted to a running account of "The History of Science at Harvard," "Scientific Instruments at Harvard before the Fire of 1764," "Instruments for the Study of Natural Philosophy after the Fire of 1764," "The Beginnings of Chemistry at Harvard," "The Biological Sciences, The Museum, and the Mineral Cabinet," and a "Conclusion." In order not to interrupt this running account, the catalog and description of the scientific apparatus is given in three appendices occupying forty-four pages of text and and twenty-four pages of plates. There is a brief foreword by Professor S. E. Morison, eleven pages of valuable bibliographical notes and references, and an excellent index.

E. C. WATSON
California Institute of Technology

Vector and Tensor Analysis. First Edition. HARRY LASS. Pp. 347+xi. Figs. 101, 16×23½ cm. McGraw-Hill Book Company, Inc., New York, 1950. Price $4.50.

This volume is the first published of the proposed International Series in Pure and Applied Mathematics. The author has set a high level of orthodox competence for the new series, and it is likely that these books will be comparable in utility and convenience to the corresponding series in physics. Collections such as these have a great responsibility, for they must serve future generations of research men as the great German *Handbücher* have served the present generation.

Writing on vector analysis today is not the hazardous occupation that it was sixty years ago. In 1892 R. B. Hayward wrote a little book called *Algebra of Coplanar Vectors* and in due time received a letter from Lord Kelvin saying: "[your book] would lose nothing by omitting the word 'vector' throughout. It adds nothing to the clearness or simplicity of the geometry . . . [vector] Quaternions came from Hamilton after his really good work had been done, and, though beautifully ingenious, have been an unmixed evil to those who have touched them in any way, including Clerk Maxwell." Kelvin, of course, was hardly an unbiased judge; he had been waging a forty-years' war with Tait to keep quaternions out of their joint "Treatise on Natural Philosophy." Not that his mind was exactly closed on the matter—he agreed to permit the use of quaternions any time Tait could convince him of their value.

From this, one might infer that Tait was a friend of vectors; not so—his love was quaternions. When Willard Gibbs published himself into the line of fire he received a broadside from Tait: ". . . Gibbs must be ranked as one of the retarders of quaternion progress, in virtue of his pamphlet on *Vector Analysis*, a sort of hermaphrodite monster, compounded of the notations of Hamilton and of Grassmann."

Well, all this was long ago and the monster has become bishop since. Professor Lass and almost everyone else use the hermaphrodite symbols without undue expectation of evil. Cayley's remark that vectors are a pocket map which has to be unfolded before use does not disturb the modern

reader who apparently can use the map without unfolding it.

In this volume the modern reader will find a good deal of physics (and mathematics, too) in vector and tensor form; about half of the chapters deal entirely with applications. Mechanics, hydrodynamics, elasticity, and electricity all provide examples of use for vector and tensor methods. The treatments of differential geometry and Einstein's law of gravitation are more complete than those usually found outside of specific treatises. Matrices are not discussed.

Each section is followed by a number of problems—there must be several hundred in all. By judicious choice of chapters to study or omit, the book may be used for a one-term course in either vector analysis or tensor analysis. There is enough material for a three-term course, but it would require nice fitting into a curriculum to avoid overlapping specific courses given in the fields of the applications.

The volume is written in the austere style apparently considered proper for mathematics these days. In this style the author's personality, if any, never intrudes. The exposition is concise and accurate and has all the bleak charm of a log table. The subject is always presented as a *fait accompli*, full blown from the brain of the past. Surely an author of a textbook has some responsibility for answering the inevitable questions which arise when the reader encounters new terms and concepts for the first time. "We call this process contraction." What is it good for? Who invented it? Why did he invent it? Has it a geometrical interpretation? Is it a generalization of something already known? Is there a process called expansion?

These questions are meant to imply that even partial understanding of a concept requires a knowledge of its intended utility, place in history, psychological origin, possibility of geometrical visualization, conceptual parentage, and the semantic overtones of its name. Most writers, faced with a mountain of material which they feel must be "covered," supply the concept only and presumably hope that the remainder will be forthcoming from the lecturer or student. The author who does answer such questions receives the thanks and patronage of those who seek not only to learn but to understand.

M. J. WALKER
University of Connecticut

Measurements of Radioactivity. LEON F. CURTISS. National Bureau of Standards Circular 476. Pp. 84+iv, Figs. 87. U. S. Government Printing Office, Washington, D. C., 1949. Price 35 cents.

Instrumentation is the main emphasis of this booklet but selected explanatory background topics such as statistical decay, modes of atomic disintegration, and properties of radiation are also treated in a helpful way. Most outstanding are the illustrative details in practical design and construction. Also desirable are the uncluttered diagrams and graphs which are often labeled with typical operating values. A full-sized book with similar features would certainly be a welcome aid to the inexperienced experimentalist. Too brief for serious reference work, the present form would serve principally as a survey for those who have a general background in physics and who would like to examine the more conventional methods for measuring radioactivity.

Outlined in the beginning are the general processes of detection. Because ionization of gases is by far the most important detecting process used today, the author gives a rather detailed description of ion discharges and their measurement. The various devices for inducing artificial radioactivity receive only enumerating mention in a brief chapter. The special techniques for studying the particular types of radiation are described tersely yet with pertinent detail. The book ends with short discussions of units of radioactivity, applications of radioactivity in biology and geology, and health protection in laboratory handling of radioactive materials. Because of their brevity the contents seem at first sight somewhat sketchy, but they are quite adequate in scope as a working guide.

J. C. LEE
Michigan State College

How to Study Physics. SEVILLE CHAPMAN. Pp. 34, $5\frac{3}{4}\times 8\frac{1}{4}$ in. Paper bound. Addison-Wesley Press, Cambridge, Massachusetts, 1949. Price $0.25 (single copy).

This handy little pocket-size booklet contains a brief discussion of some general methods of study together with suggestions regarding specific activities such as the taking of lecture notes, problem solving, laboratory work, preparing for and taking examinations.

The discussion of problem solving should be particularly helpful to students since it contains, in addition to a rather detailed plan, such valuable suggestions as reworking the problem backward from the answer, repeating with modifications, looking for fundamental principles rather than trying to memorize specific situations, and trying to judge the reasonableness of a numerical answer. Many other fruitful techniques such as frequent "self-recitation," the making up of specimen examination questions, and careful correlation of laboratory work with lecture and recitation, are suggested throughout the book. In his preface the author says, "Every suggestion included here has been of use to someone." In view of this assurance and of the very modest price many teachers of general physics will want to recommend this booklet to their students.

W. W. MCCORMICK
University of Michigan

The General Principles of Quantum Theory. Fourth edition. G. TEMPLE. Pp. 120+viii, $4\frac{1}{4}\times 6\frac{3}{4}$ in. Methuen and Company, Ltd., London, 1948. Distributed in the United States by John Wiley and Sons, Inc., New York. Price $1.25.

Books on quantum mechanics are of two kinds: those that develop the Schroedinger equation and apply it to specific problems, and those that undertake to set forth the abstract principles of quantum mechanics. This book is of the latter kind. Its material has been drawn from the

well known works of Dirac, Heisenberg, Neumann, and Weyl on the subject.

The book starts with a discussion of the mathematical apparatus of quantum theory, that is, of linear operators and complex vector spaces. Then follows a short exposition of the quantum philosophy of measurement of microphysical phenomena. The rest of the book is devoted to showing how the mathematical apparatus can be used to express quantitatively this philosophy of measurement; the development includes the formulation of the uncertainty principle, the exchange relations, the equations of motion, the representation of vectors by means of the spin operators, and the quantum theory of composite systems. Throughout the book numerous examples are provided on which the reader may test his skill.

Because of the nature of the subject, the book is not easy to read; but it is a very attractive work because of its logical development, its gradual progression from less difficult to more difficult matters, and the extreme purity of its English. The first edition was published in 1934, and it is probably fair to say that it presented the material in considerably better digested form than earlier works that appeared immediately after the invention of quantum mechanics. The later editions have undergone only minor changes, but many readers will still find the book more satisfying than some of the better-known treatises on the subject.

W. T. PAYNE
Michigan State College

New Members of The Association

The following persons have been made members or junior members (*J*) of the American Association of Physics Teachers since the publication of the preceding list [*Am. J. Physics* **18**, 532 (1950)].

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Author Index to Volume 18

In this index are listed the names of authors and the titles of their articles and notes. Abstracts of papers read at meetings are generally not listed; however, they are indexed in the Analytic Subject Index. Those which are included in the Author Index are designated by the symbol (D). The symbol (L) after an entry signifies a Letter to the Editor.

Analytic Subject Index to Volume 18

The titles of articles are disregarded, the entries being based on analyses of the contents of the articles. The symbol (D) designates a digest of an article or an abstract of a paper read at a meeting; (T) designates title only and (L) designates a Letter to the Editor.

To facilitate reference to any desired subject, the index is divided into sections arranged alphabetically. The titles of these sections and of cross references to them are as follows: